THE AMERICAN
JOURNAL OF PSYCHOLOGY
Founded in 1887 by G. STANLEY HALL

Vol. LIII JULY, 1940 No. 3

THE RELATION OF PITCH TO FREQUENCY: A REVISED SCALE

By S. S. STEVENS, Harvard University, and J. VOLKMANN, Columbia University

That *pitch* is not the same as *frequency* has already been amply demonstrated.[1] That pitch is a *function* of frequency is a fact too obvious to need argument. How pitch is related to frequency, however, has long been a vexatious problem disputed by musicians and psychologists alike. The task here undertaken is that of investigating the dependence of pitch upon frequency for the purpose of erecting a scale by which pitch, as a perceived aspect of tones, can be measured.

Pitch is one of the psychological aspects, or attributes, of tone. It is one of the dimensions in terms of which we are able to distinguish and classify auditory sensations. Pitch differs from frequency in that pitch is determined by the direct response of a human listener to a sound-stimulus, whereas frequency is measured with the help of instruments. A *pitch-scale* is what we obtain when we assign numerals to tones in such a way that the numerals stand in some definite relation to the pitch exhibited by the tones. For the assignment of numerals to tones many schemes are possible, and we shall begin with a look at some of these alternatives.

SCALES IN GENERAL

To any aspect of an object or event that exhibits itself in discriminable degrees we can assign numerals. Human beings can tell one numeral from another and most of them know the relative positions of numerals in the numeral-series. Consequently, when different degrees of the same aspect can be discriminated, these manifestations of the aspect can be

* Accepted for publication August 21, 1939.
[1] S. S. Stevens and H. Davis, *Hearing: Its Psychology and Physiology*, 1938, 70.

BOSTON UNIVERSITY
COLLEGE OF LIBERAL ARTS
LIBRARY

paired with different numerals, and a kind of scale is at once achieved. *Numerals* and not numbers are what we assign when we make a scale, for number is itself an aspect of things, like weight; and for both number and weight we pair numerals with things in order to make scales.[2] But the assignment of numerals to aspects is a trivial enterprise unless some sort of definite rule is followed. In actual practice we use many different rules, and, because of the differences among the rules, we have different kinds of scales and different kinds of measurement.

We shall here consider three kinds of scales, gotten by three different rules for the assignment of numerals. The numerals, if we may recall some obvious points, are signs which we commonly make on paper, for example: 1, 2, 3, 4, etc. From the point of view of the scale-maker, the important thing about the numerals is not their shape or their history—it is their position in the numeral-series. In fact the usefulness of numerals in measurement and in mathematics derives directly from the fact that each numeral has a definite position in a series and hence bears definite 'spatial' relations to the other numerals. These 'spatial' relations among the numerals figure, in various ways, in the construction of the following types of scales.

(1) Ordinal scale. Given several objects or events exhibiting various degrees of a particular aspect, we can pick out the least and the greatest degree, and rank-order the other cases between these extremes. Then, having thus ordinalized the objects, we can assign them numerals in such a way that the order ('spatial' order) of the numerals reflects the rank-order of the objects. To the rank-ordered objects we could assign the numerals 1 2 3 4 . . . or 2 4 6 8 . . . or 12 17 32 37 . . . or any other set of numerals whose *order* is determined. We could use the letters of the alphabet too if we liked, for an ordinal scale tells us when one thing is greater than, equal to or less than another as regards some particular aspect, but it does not tell us how much greater it is. In other words, the property of order within the numeral series is the only property utilized in an ordinal scale.

(2) Intensive scale. Whenever, in addition to knowing how to order a set of items, we are able to devise a rule telling us when to assign the numeral *adjacent* to the one just previously assigned, we possess the makings of what we might call an intensive scale. Thus, in erecting a psychological scale for a series of grays, we should have an intensive

[2] For a discussion of the relation of the numeral-series to scales, see S. S. Stevens, On the problem of scales for the measurement of psychological magnitudes, *J. Unified Science* (in press).

scale provided we assigned adjacent numerals to grays separated each from the next by a just perceptible difference. Gray number 17 would be just noticeably lighter than gray number 16, and just noticeably darker than gray number 18.

It might appear that we have here a scale with a *unit*. Fechner thought so and many have agreed with him. It must be pointed out, however, that a rule for assigning adjacent numerals to objects or events does not necessarily create a unit of measurement. For one thing a unit of measurement must remain invariant. The subjective size of the jnd for loudness has been shown to vary tremendously,[3] but, as we shall see later, the jnd for pitch appears to remain subjectively constant at all frequencies. Hence, many scales of the sort Fechner proposed would seem to be intensive scales only, possessing a definite order and a rule for the assignment of adjacent numerals.

The scale of musical notes *do, re, mi, fa, . . .* can also be considered an intensive scale: the notes themselves are ordered, as are numerals, and adjacent notes are assigned to frequencies bearing a certain *ratio* to one another. The musical note, however, is not a *unit* in the strictest sense.

(3) Extensive scale. The extensive scale utilizes more of the properties of the numeral-series than either of the two previous scales. In addition to the factors of order and adjacency, we here encounter the property of 'distance' or extent. The numeral 8 is as far from 4 as it is from 12, and it is half as far along the numeral series as is 16. Incorporating these properties in a scale, we assign numerals in such a way that the relations among them reflect relations among the objects. Thus we assign the numeral 16 to a length that is twice as long as that to which is given the numeral 8, and so on. Similarly, in scaling an attribute of sensation, if we can devise rules so that the numeral 8 is assigned to the case exhibiting half the magnitude of that to which 16 is assigned, we can erect an extensive scale wherein the 'spatial' relations among the numerals correspond to perceived relations among sensations. This type of scale allows us to define and utilize a *unit* and to employ the rules of arithmetic in manipulating the scale-numerals.

Extensive scales are generally the most useful scales for science, and the generation of an extensive scale for the attribute of tonal pitch is the aim of the present experiment.[4]

---

[3] S. S. Stevens, A scale for the measurement of a psychological magnitude: Loudness, *Psychol. Rev.*, 43, 1936, 405-416.

[4] What is here called an *extensive* scale is the same as what has sometimes been termed a *numerical* scale. The present terminology seems preferable, however, since almost all scales are numerical scales in some sense of the word.

The Previous Experiment

In the summer of 1936 Stevens, Volkmann, and Newman presented a group of *O*s with the task of adjusting the frequency of one of two alternately presented tones until it sounded half as high in pitch as the other tone.[8] The *O*s found the adjustment easier to make than might be supposed, and from their results it was possible to construct an extensive scale of pitch. The unit chosen for the scale was called a *mel* and the pitch, in mels, of each audible frequency was represented by a numeral in such a way that the 'spatial' relations among the numerals were reflected in the perceived relations among the pitches.

Although this earlier experiment demonstrated the feasibility of erecting a subjective scale for pitch and revealed certain interesting relations between the form of such a scale and functions pertaining to differential sensitivity and cochlear mechanics, several problems remained unsolved. First, there was the question as to whether a scale obtained by the method of fractionation (setting one tone to half the pitch of another) could predict, as it should, the results of an experiment on the bisection of tonal intervals or the judgment of equal sense-distances. In other words, could the same scale be got by different experimental approaches?

Secondly, we need to know more about the reliability of the judgments upon which a pitch-scale must be based. How does the variability among successive judgments by a single *O* (intra-observer variability) compare to the variability among the averages obtained from different *O*s (inter-observer variability)? The answer to such questions requires the services of more *O*s than coöperated in the earlier experiment.

Thirdly, given better experimental apparatus, what changes would appear in the results? The earlier apparatus was quite adequate for all except the very low tones. Below a frequency of about 120 ~, however, the intensity of the tones fell off so rapidly with decreasing frequency, that the behavior of pitch in the region of the lowest tones could not be adequately explored. We suspected that the pitch-scale as previously published needed correction at its lower end.

Finally, what of the complaint made by some *O*s that they were uncertain as to what is "zero pitch"? The two *O*s who imagined a zero equal to "the lowest note on the organ" deviated systematically from the other *O*s who, according to their introspections, thought little about the problem of zero pitch. Perhaps an experimental method which acquainted *O* with zero pitch directly would improve the reliability of the experiment and make a systematic difference in the results.

The following experiments were directed upon these problems.

Apparatus for Determining Equal Sense-Distances

In order to present the *O*s with working conditions as nearly optimal as possible, we designed what is essentially an electric piano, consisting of 20 wooden keys each an inch wide (see Fig. 1). Each key, when pressed, produced a pure tone in a loud speaker. Above each key and immediately in front of *O* was a small knob for controlling the frequency of the

[8] S. S. Stevens, J. Volkmann, and E. B. Newman, A scale for the measurement of the psychological magnitude pitch, *J. Acoust. Soc. Amer.*, 8, 1937, 185-190.

tone produced by its respective key. Thus 20 different tones were available at the touch of a key, and the frequency of each of these tones could be independently adjusted throughout a wide range. The total range of the piano was from 0 to 15,000 ~ per sec.

A few technical details may be noted. The pressing of a key introduced into the circuit on the 'fixed' side of a beat-frequency oscillator (General Radio, type 613-B) an amount of capacitance determined by the setting of a variable condenser connected to the key. The knob above each key controlled this variable condenser, whose

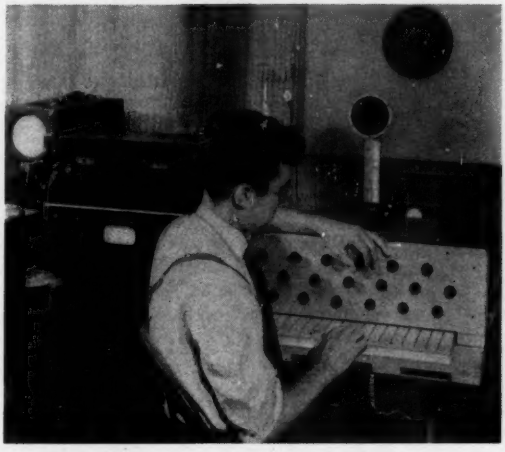

FIG. 1. THE APPARATUS

size was chosen to fit the range of frequencies that the key was meant to cover. With no keys pressed, the beat-frequency oscillator was tuned to zero by turning its main tuning condenser, and in this condition no tone could be heard.

The output of the oscillator was led to an amplifier containing a so-called equalizing network. The important function of this amplifier was to keep the intensities of all tones adjusted so that tones of all frequencies sounded approximately equal in loudness. The loudness-level used was about 55 db. The equalizing network was essentially a variable filter, by means of which the frequency-characteristic of the amplifier could be altered. In order to compensate for the characteristics of the loud speakers and to maintain constant loudness throughout the frequency-range, we found that the response of the amplifier had to be increased by about 26 db. at 25 ~ and by about 24 db. at 10,000 ~. Under these conditions the *O*s made no complaints about some tones being louder than others. When there are prominent differences in loudness, the *O*s seldom fail to mention the fact, because it distracts attention.

The loud speaker was a 13-in., permanent-magnet type (Cinaudograph). It was

mounted in a large baffle designed according to the bass-reflex principle, and its natural period was as low as 30 ~. The loud speaker was suspended at one end of a room lined with rock-wool. *O* sat facing the speaker 7 ft. in front of it. In order to augment the high tones (above 8,000 ~) the large speaker was supplemented by a small 'crystal' speaker suspended 3 ft. in front of *O*.

The frequency of the tone produced by each key of the electric piano could be read from the dial of a wave-analyzer (General Radio, type 736-A) which was connected in parallel with the loud speakers.

The Pitch Scale Determined by Equal Sense-Distances

Preliminary experimentation convinced us that it is impracticable to ask an *O* to adjust the frequencies of all 20 keys so as to create 19 equal intervals of pitch between, let us say, 40 and 12,000 ~. It is also rather wasteful of effort to try to divide such a wide range—40 to 12,000 ~—into only two equal intervals of pitch. After several exploratory tests, we concluded in favor of the following procedure.

Five keys were selected from the piano; the lowest key was tuned to 200 ~ and the highest to 6500 ~, and *O* was required to adjust the frequencies of the three intermediate keys until the pitch-distance between each pair of adjacent keys appeared equal. In other words, the *O*'s problem was to produce four equal intervals of pitch. He was allowed as much time as he desired, but he was instructed not to pass on his final adjustments until he had carefully compared each interval with each other interval, playing the intervals in both ascending and descending order. This requirement was meant to discourage *O* from being too easily satisfied with only successive bisections; he had to perfect his adjustments even after he had bisected the initial long pitch-interval, and had bisected each of the resulting halves.

The same procedure was carried out for two additional ranges of frequency: 40 to 1000 ~ and 3000 to 12,000 ~. Each *O* made, at different times, five settings for each of the three ranges, and 10 *O*s took part in the experiment.

It will be noted that none of these ranges can be divided into four equal intervals in such a way that the intervals are musical octaves or fifths, the two musical intervals of simplest ratio. This choice of range was designed to remove from the musical *O* any temptation to adjust the tones to musical intervals. It appears, however, that this precaution is not entirely necessary—when they are directly compared, octaves in different parts of the musical scale do *not* sound like equal intervals or extents of pitch. Rarely did the *O*s report a desire to adjust the frequencies to musical ratios, or appear frustrated because it was impossible.

Table I records the results of this experiment for the 10 *O*s. For each *O* and for each of the three ranges of frequency, the three frequencies used to divide the total range into four equal intervals are tabulated. Each entry is the average of five settings. Measures of variability are also recorded. These will be discussed later, but let us now proceed to the construction of a scale of pitch.

Assuming that the *O*s have obeyed instructions and that, subject to

a certain variability, they have divided the interval between 200 and 6500 ~ into four equal pitch-distances, we can proceed directly to plot a function between these two limits. This function is the section of the curve in Fig. 2 which is determined by the circles. (The filled circles mark the ends of the range; the hollow circles show the average adjustments by the 10 *Os*.) These circles determine a function—slightly sigmoid in form—between two limits. In plotting this section of the curve, we have had to meet only the requirement that the vertical distances

TABLE I

DATA ON EQUAL SENSE-DISTANCES

Entries give the adjustments made by 10 *Os* in dividing three different frequency ranges into four equal intervals of pitch. Each entry is the average of five settings. The bottom row gives the percentage average deviation, *V*, of the means for different *Os* about the mean of the group. The mean of this row is 6.5%—the inter-observer variability. The last column gives for each *O*, the mean of the percentage average deviation, Vo, of his own settings about his own means. The mean of this column is 9.9%, the intra-observer variability.

O	40–1000 ~ per sec.			200–6500 ~ per sec.			3000–12000 ~ per sec.			Av. Vo (%)
JV	174	381	628	865	1850	3260	4090	6780	8440	9.4
SS	170	454	718	792	2230	3760	3970	5670	8080	8.6
GS	164	327	598	822	2060	3620	4090	5130	7290	6.7
CH	167	392	706	751	1696	3270	3820	5260	7540	8.7
JB	171	442	718	920	1810	2880	4620	5800	8320	11.9
DM	168	391	768	972	1920	3320	4100	5110	7840	5.4
DC	142	490	716	848	1859	3120	4410	5700	7480	12.7
AB	140	385	732	985	2390	3850	3940	5260	7120	12.2
AS	161	404	642	864	2340	3420	3840	5110	7580	13.8
FS	154	376	706	846	2065	3430	4210	5440	7740	10.3
Mean	161	404	693	867	2022	3393	4109	5526	7743	
V(%)	5.9	8.6	6.1	6.4	9.6	6.6	4.4	6.7	4.4	

between successive circles are equal—since vertical distance on the graph represents pitch-distance as perceived by a listener.

Next we ask what form the function has outside the range 200 to 6500 ~. As shown in Table I, *Os* also divided into four equal intervals another range of frequencies (40 to 1000 ~) partially overlapping the first range. If, within this second range also, the *Os* have validly produced four equal intervals of pitch, then the settings in this range (indicated by the squares in Fig. 2) must be plotted in such a way as to meet two requirements: (1) the vertical distances between successive squares must be equal; and (2), throughout the range common to the two functions, the curve determined by the squares must coincide with the curve determined by the circles. If these two requirements can be met—as it appears in Fig. 2 they can be—then we can extend our function down to 40 ~.

Subject to the same two requirements, the data for the range 3000 to 12,000 ~ can next be plotted, so as to extend the function to the higher frequencies. It should be noted here that an important criterion for the validity of the data expressing equal sense-distances (Table I) is met

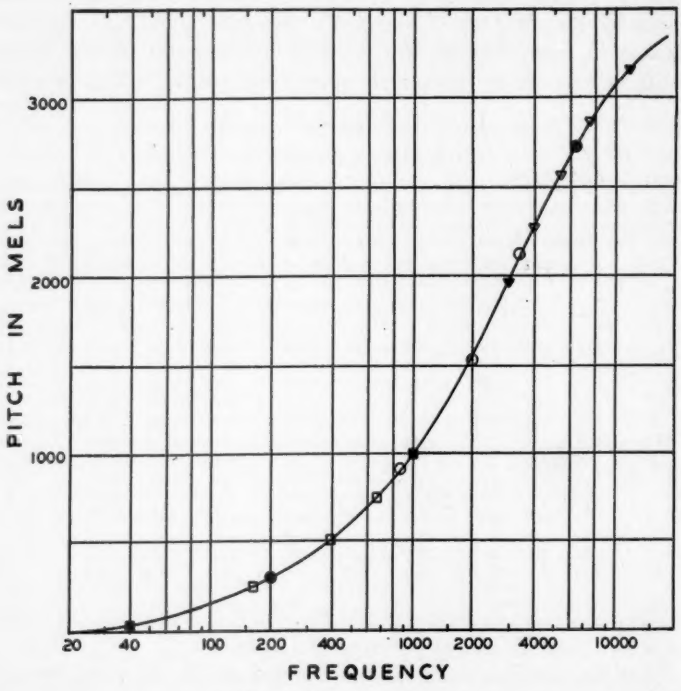

FIG. 2. THE PITCH-SCALE

The curve shows how pitch, scaled in subjective units (ordinates), varies with frequency. The circles, squares and triangles represent data obtained in the experiment on equal sense-distances. The filled figures mark the ends of three frequency-ranges and the hollow figures show the points arrived at when the *O*s divided the ranges into four equal intervals of pitch.

and satisfied when it is shown that the data can be plotted in such a way as to meet simultaneously the two requirements stated above. How well these requirements can be met by the data of the present experiment the reader will discover from Fig. 2. Or we may say: the fact that the data can be plotted as shown in Fig. 2 demonstrates that, even when the *O* is made to work with different ranges of frequency, the rate of change of

pitch with frequency is a function of the frequency and not of the range bisected, or quartered. In other words, the *slope* of the curve at each frequency within the region of overlap can be made the same for either of the two sets of overlapping data chosen from the three sections of Table I.

In constructing the curve between 40 and 12,000 ~ in such a way as to satisfy simultaneously the two requirements previously stated, we proceed by purely graphical methods involving no small amount of trial and error. The aim is to find the curve that will best represent all the data. The final construction is a curve whose ordinate has this property: equal distances along the ordinate represent equal pitch-distances. Such a curve could serve, between 40 and 12,000 ~, as an *intensive* scale of pitch, but it does not yet meet all the requirements of an *extensive* scale. It needs a zero point. The location of zero pitch on the ordinate of Fig. 2 can be determined in two ways: (a) by extrapolating the function to the frequency marking the lower limit of pitch perception, or (b) by making a direct estimate of the amount of pitch left in the region below 40 ~. Fortunately, in the present experiment both methods lead to the same result, provided we accept 20 ~ as approximately the lowest frequency giving rise to a sensation of pitch.[6] In order to test the validity of the extrapolated curve, 7 of the *O*s equated the pitch-distance between 25 and 40 ~ to an equal-appearing interval above 40 ~. In order to appear equal, this upper interval had, on the average, to extend from 40 to 52 ~. This result agrees with what we should predict from the extrapolation of the pitch-function to 20 ~. Consequently, we can extend the curve in Fig. 2 down to what we may reasonably call zero pitch.

Now we are in a position to choose a unit for our scale of pitch—an extensive scale at last. We can assign numerals to positions along the ordinate of Fig. 2 in such a way that the numerals, and their series-relations, will reflect the relations among perceived pitches. Accordingly, we may assign the numeral 1000 to the ordinate corresponding to a frequency of 1000 ~, and this numeral we may allow to stand for the number of *mels* in the pitch of a 1000 ~ tone. The *mel,* then, is a unit of pitch, and the magnitude of the discriminable aspect, pitch, exhibited by tones of various frequencies can be read from the plot in Fig. 2.

[6] For a discussion of the threshold for very low tones, see E. G. Wever and C. W. Bray, The perception of low tones and the resonance-volley theory, *J. Psychol.,* 3, 1936, 101-114; G. von Békésy, Über die Hörschwelle und Fühlgrenze langsamer sinusförmiger Luftdruckschwankungen, *Ann. d. Physik,* 26, 1936, 554-566.

Relation of This New Scale to the Earlier Scale of Pitch

In the scale constructed by Stevens, Volkmann, and Newman,[7] the mel appeared as the unit and was defined in the same way: as one thousandth of the pitch of a 1000 ~ tone. The function relating pitch to frequency, however, turned out to be slightly different from that pictured in Fig. 2. Plotted in the coördinates of Fig 2 the two curves would coincide at 20 and at 1000 ~, but between these two frequencies the earlier curve would lie below, and above 1000 ~ the earlier curve

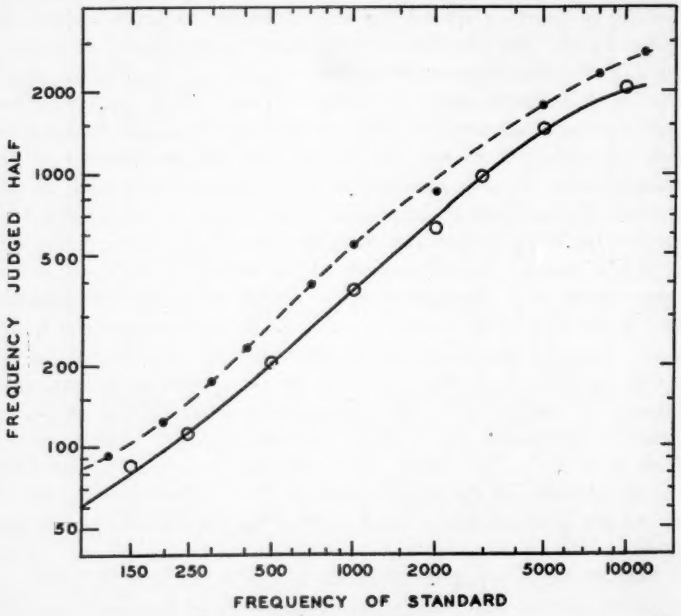

Fig. 3. Fractionation

The results of two different experiments in which *Os* adjusted the frequency of one tone (ordinates) until it sounded half as high in pitch as a second tone (abscissas). The points on the dashed curve are for data obtained in 1936. The solid curve gives the locus of fractionations as they are predicted from the curve in Fig. 2. The hollow circles indicate the data obtained in the present experiment.

would lie above the curve shown in Fig. 2. It was partly in order to discover, if possible, the reason for this discrepancy that the next experiment was designed.

The nature of the discrepancy between the two pitch-functions is best shown in Fig. 3, where are plotted the data of the earlier experiment (dashed line) showing what frequencies were judged half as high in pitch as what other frequencies. The frequencies that would have been heard as half as high, *provided* Fig 2 contains the

[7] Stevens, Volkmann, and Newman, *op. cit.*

true pitch-function, are represented by the solid curve in Fig. 3 (let us ignore, for the present, the hollow circles). This solid curve, in other words, gives the predicted results for an experiment on fractionation.

It will be seen that in the earlier experiment, the frequencies judged half as high in pitch were systematically too high. Now, a study of these earlier results shows that the two *Os*, who made their fractionations in terms of a zero pitch which was definitely conceived as the lowest note on the organ, made fractionations which agree not too badly with the solid curve in Fig. 3. Perhaps, then, if the *Os* were allowed to hear a pitch closely approximating zero they would produce fractionations agreeing more closely with the results predicted by the experiment on equal sense-distances.

A New Experiment on the Fractionation of Pitch

In this experiment *O* was required to adjust the frequency of one tone until it sounded half as high in pitch as another tone of pre-

TABLE II

Data on Fractionation

Entries give the results for 12 *Os* who adjusted one tone to half the pitch of various standard tones. Each entry is the average of five settings. The next to the last row gives the percentage average deviation, V, of the means for different *Os* about the mean of the group. The mean of this row is 9.4%—the inter-observer variability. The last row gives the mean of the percentage average deviations, Vo, of the individual settings about their own means. The mean o this row is 15.6%—the intra-observer variability.

O	Standard tones							
	150~	250~	500~	1000~	2000~	3000~	5000~	10,000~
JV	72	105	152	396	508	870	1166	1740
SS	80	108	232	338	577	1012	1358	2260
GS	81	110	174	356	546	1164	1380	1800
CH	89	120	190	376	664	1105	1980	2480
JB	91	125	214	416	724	1108	1206	2090
DG	82	104	210	388	494	856	1436	2120
ED	81	115	224	346	760	1118	1630	2090
JN	93	102	233	446	620	836	1226	2005
DM	92	116	252	400	594	—	1520	2520
WH	91	113	207	373	724	—	1566	2057
DC	81	103	197	261	638	—	1500	1850
AB	84	116	182	384	748	—	1270	1760
Mean	85	111	206	373	633	1009	1437	2064
V (%)	5.9	5.4	10.7	8.9	13.6	9.3	11.8	9.5
Av. Vo % for all *Os*	8.0	10.8	17.2	16.1	19.6	14.2	21.1	17.7

determined frequency. The two tones were presented alternately by a timing relay at 2-sec. intervals. Each tone lasted 2 sec. and was followed immediately by the other tone. *O* controlled the frequency of the second tone by turning a knob connected to a variable condenser which tuned a beat-frequency oscillator. The oscillator, amplifier and speakers were the same as were used in the experiment on equal sense-distances. The pro-

cedure was the same as that used in the earlier experiment, except for this crucial difference: *O* could, whenever he desired, turn a knob and introduce a 40 ~ tone. He was told that this tone was at very nearly zero pitch, and that he should make his fractionations with that in mind. In practice, the *O*s usually referred to the 40 ~ tone two or three times in the course of each fractionation.

The error introduced by the fact that 40 ~ is not precisely zero pitch is certainly negligible when high tones are being fractionated, but the same is not true for very low tones. Consequently, when the *O* was asked to fractionate a tone of 150 ~, he was not allowed to use 40 ~ as a reference tone.

The results obtained from 12 *O*s are shown in Table II. Each entry is the average of 5 settings. All but one of the 5 *O*s employed by Stevens, Volkmann, and Newman[8] served also in this experiment. These *O*s expressed the belief that the presence of the 40 ~ tone made the judgment easier, and, as we might expect, the agreement among the results was better when the reference tone was provided. Not only did the results agree better among themselves, but they also agreed better with the solid curve of Fig. 3. The hollow circles in Fig. 3 represent the means of the columns in Table II.

Two facts are strikingly in evidence: (1) the availability of a 'zero' for purposes of reference tends to lower the fractionation—a result which suggests that, without an objective reference, the *O*s tend to assume a zero pitch that is too high; and (2) the results obtained by the method of fractionation adequately confirm those gotten by the method of equal sense-distances. *The agreement of these two methods leads us to propose the function represented in Fig. 2 as the scale most acceptable, at present, for relating pitch to frequency and for measuring subjective pitch.*

Let us now consider certain minutiae. First of all it might be objected that the introduction of the 40 ~ tone transforms the experiment on fractionation into one on bisection. Perhaps this is so. We shall not quarrel with anyone wishing to use the latter term. Considered formally (logically), a fractionation to the value one-half is equivalent to a bisection between some higher point and zero. In any case, the procedure in this second experiment differed from that of the experiment on equal sense-distances in two important ways: (1) *O* used the reference tone only occasionally; and (2) he was instructed to use it as a reference tone, aiding him in his task of fractionation, and was instructed not to use it as a limiting tone in a self-imposed task of bisection. Despite these differences in procedure, the results of the two experiments are in striking agreement.

The fractionation of the 150 ~ tone, where the 40 ~ reference tone was not

[8] Stevens, Volkmann, and Newman, *op. cit.*

used, occurs at a lower frequency than we should have predicted on the basis of the earlier experiment. This difference is most likely due to improved apparatus. The earlier experiment suffered from the lack of devices capable of producing very low tones at a constant loudness-level. In the present experiment the *O* was given the opportunity of exploring the lower ranges of frequency down as far as 25 ~.

The fractionations of the 2000 ~ tone call for special attention. In both experiments this point is out of line (see Fig. 3). An examination of the individual adjustments shows what appears to be an explanation: in fractionating 2000 ~ the *O*s tend to set the second tone to one of the octave relations, either 500 or 1000 ~. There is a large cluster of results near 500 ~ and a smaller one near 1000 ~. The *O*s appear to be unaware of this tendency, and at no other frequency did such a tendency show itself. Upon discovering this anomaly at 2000 ~, we obtained judgments at 3000 ~ from as many *O*s as were still available, but found no tendency for the individual results to cluster about the lower octaves. All we can say is that the octave, or double octave, relation asserts itself when *O* is seeking a tone that sounds half as high in pitch as a tone of 2000 ~, but that these octave relations do not affect his judgment at other frequencies.

The Question of Variability

The precision with which we can assign numerals to things depends, of course, upon our powers of discrimination, and these in turn are reflected in the variability of our judgments. The reliability of the scale of pitch here proposed depends, then, upon the variability of the settings made by the several *O*s. Table I shows what this variability amounted to when the *O*s were asked to divide an interval into four equal sense-distances. The variabilities are expressed in terms of the coefficient of variability (V)—the average deviation taken as a percentage of the mean. The bottom row of Table I tells us the variability found when we compare the average results for different *O*s (inter-observer variability), and the right-hand column gives the average of the variability shown by each *O* in making his own frequency-adjustments (intra-observer variability). The average intra-observer variability is 9.9%; the average inter-observer variability is only 6.5%. The relation between these two figures we will discuss shortly.

In the experiments on fractionation the variability was greater, as we should expect, than in the experiment on equal sense-distances. Table II gives the values for fractionation. Here the average intra-observer variability is 15.6% and the average inter-observer variability is 9.4%. In the earlier experiment, where fewer *O*s and no reference tone for zero pitch were used, the average inter-observer variability was 14.9%. When we compare directly the results of the four *O*s who took part in both experiments, we find that the procedure of the later experiment was effective

in reducing the inter-observer variability by about 57%. Consequently, for an *O* whose task it is to fractionate a tone, an adequate point of anchorage for his own subjective scale appears to be definitely required.

There is a tendency for the average deviation to increase when the tone to be fractionated is made higher in frequency. This increase appears to be no simple function of the frequency, but on the assumption that the increase might be related to the subjective pitch of the tone fractionated, the variabilities shown in Table II were translated into

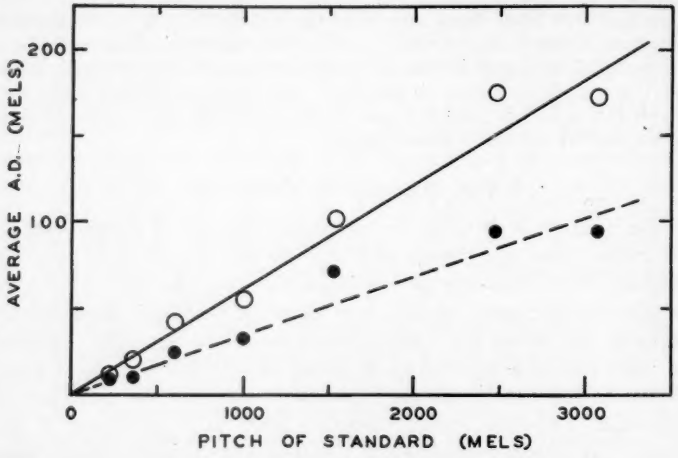

FIG. 4. VARIABILITY

The variability of fractionations depends upon the pitch of the tone fractionated. Both the average deviations and the pitches of the tones are expressed in subjective units (mels). The hollow circles show the intra-observer deviations; the filled circles show the inter-observer deviations.

subjective units and plotted in Fig. 4. The intra-observer average deviations, measured in mels, are plotted as hollow circles; the filled circles represent the inter-observer average deviations. The transformation of variabilities into deviations expressed in mels was accomplished with the aid of Fig. 1, or, more correctly, with the aid of its enlarged prototype. Fig. 4 indicates that the extent, in pitch, of the average deviation to be expected when an *O* fractionates a tone is approximately proportional to the perceived pitch of the fractional tone. Likewise, the variations among different *O*s are directly proportional to the subjective pitch of the tone with which they are working.

One more interesting relation emerges from the data on variability. The ratio of the inter-observer to the intra-observer variability is 0.66 for the experiment on equal sense-distances and 0.61 for the experiment on fractionation. Now, if our assumption is correct that all *O*s are carrying out the same task, as set for them, and if we assume that the deviations of their settings from the 'true' value are due to 'chance' factors, and not to the fact that different *O*s do different things when asked to judge pitch, then we can apply certain notions derived from the theory of sampling. There is a formula relating the variability in a small sample chosen at random from a normal distribution and the variability of the means of several small samples so chosen. It is the familiar formula for the reliability of the mean: $AD_m = AD/(N\text{-}c)^{\frac{1}{2}}$. Applied to the present experiment, AD is the intra-observer variation, AD_m is the inter-observer variation, N is the number of settings made by each *O* for a given tone, and c is a factor dependent upon the size of N. The value of c for N less than 10 is usually taken as 3, but this is admittedly only an approximation. For $N = 5$ and $c = 3$ this formula says that the ratio of the inter- to the intra-observer variability should be 0.707—a value reasonably close to the two values cited above.

In making this comparison between the ratio predicted and the ratio obtained, we have lumped the variabilities for all *O*s at all frequencies. Nevertheless, despite a possible obscuring of minor irregularities, it is probably safe to conclude that this analysis of variability supports the assumption that the *O*s were operating under the same set and *Aufgabe* and that their deviations were due mainly to chance factors. At least a necessary condition for such an assumption has been fulfilled. If each *O* had been performing under a different attitude—some *O*s trying to adjust to various musical intervals, others to equal pitch-distances, for example—the ratio of inter-observer to intra-observer variability would most likely have been larger than 0.707 instead of smaller.

Agreement with Antecedent Experiments

The data plotted in Fig. 2 do not, of course, represent the first attempt of psychologists to establish equal intervals of perceived pitch. Other experimenters have bisected and equated pitch-distances. Consequently, it is instructive—and in this case somewhat comforting—to compare the results of previous workers with the function in Fig. 2. A section of this function (extending from 100 to 1000 ~) is reproduced in Fig. 5 and with it are plotted data obtained by Pratt, Münsterberg, and Lorenz in experiments on the bisection of tonal intervals.

Lorenz[9] performed the experiment which launched the historic feud between Wundt and Stumpf.[10] He found that the bisection of the interval between two frequencies occurred at or near the arithmetic mean of these frequencies. Wundt stamped these results with his approval, but Stumpf

[9] C. Lorenz, Untersuchungen über die Auffassung von Tondistanzen, *Philos. Stud.*, 6, 1890, 26-103.

[10] E. G. Boring, *A History of Experimental Psychology*, 1929, 371.

dissented. Stumpf, the musician, held that the bisection should be nearer the geometric mean—the point which divides an interval into two equal *musical* intervals. Our present pitch function (Fig. 2) suggests that both parties to the controversy were correct, provided one is free to choose the range of frequencies to be bisected. In the region where the curve (Fig. 2) is rectilinear, or nearly so (from about 1500 to 6000 ~)

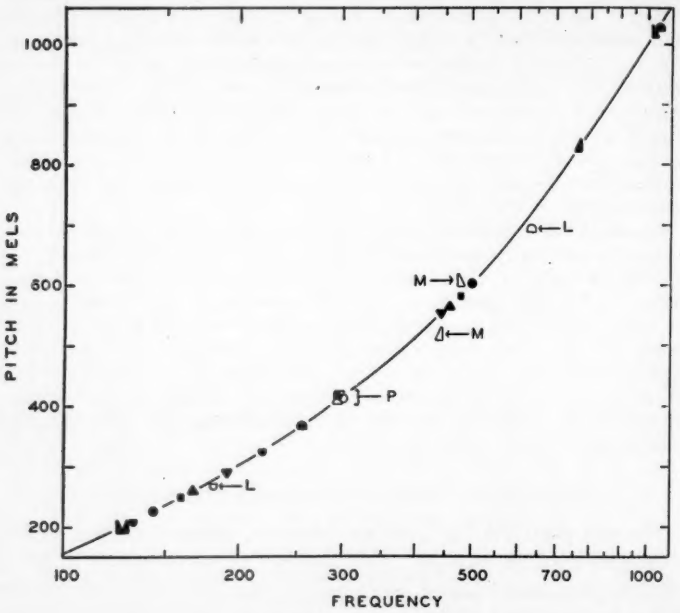

FIG. 5. HISTORICAL COMPARISON

Comparison of the pitch-function with data on the bisection of intervals obtained by Pratt (P), Münsterberg (M), and Lorenz (L). The hollow figures mark the points of bisection. The ends of the interval bisected are marked by filled figures of corresponding shape.

Stumpf's contention is borne out in experiment. In this region bisections are made at or near the geometric mean and equal musical intervals sound approximately equal in pitch-extent. But in the region below 1500 ~, bisections are made slightly nearer the arithmetic than the geometric mean, although they seem never to be made quite at the arithmetic mean. This point is illustrated in Fig. 5 where two of the longer intervals used by Lorenz are plotted. The points of bisection are

marked L, and it will be seen that these points lie near but definitely below the pitch-function.

Münsterberg set out to settle the issue with some bisections of his own.[11] His results appeared to him to confirm Lorenz, but when we plot Münsterberg's data for the longer intervals we obtain the result shown in Fig. 5. The figures marked M represent these bisections. Both bisections fall very near the point predicted by the pitch-scale; and, unlike Lorenz's data, the deviations are in both directions. It should be noted too that we have plotted the data for the two longest intervals employed by Münsterberg; his results for shorter intervals fall even closer to the curve in Fig. 5.

Further support for the validity of our present pitch-scale is found in Münsterberg's experiment in which he equated two pitch-extents that

TABLE III

A COMPARISON

Data obtained by Münsterberg are here compared wtih the values predicted by the pitch-scale.

Standard interval 256~ to —	Obtained interval — to 456~	Predicted interval — to 456~	Size obtained interval in mels	Size predicted interval in mels
276	426	430	22	23
296	406	406	44	44
316	388	388	63	63
336	374	366	80	75

were not adjacent.[12] The ends of these two extents were defined by four different tones, as shown in Table III. Here we can compare the results obtained with what we should predict from the pitch-function in Fig. 2. Two of Münsterberg's four intervals check perfectly; the other two deviate slightly, and in opposite directions, from the predicted values.

Perhaps the most adequate of the previous experiments, from the point of view of apparatus and technique, is that of Pratt.[13] He used four intervals, each larger than an octave, and required his *O*s to bisect the intervals by themselves adjusting the frequency of the bisecting tone. The results are shown in Fig. 5 by the group of points marked P. It is clear that the present pitch-scale confirms the results of Pratt as closely as experimental error would allow us to expect.

[11] H. Münsterberg, *Beiträge zur experimentellen Psychologie*, 4, 1892, 147-177.

[12] For these results see E. B. Titchener, *Experimental Psychology*, vol. 2, part 2, 1905, 243.

[13] C. C. Pratt, Bisection of tonal intervals larger than an octave, *J. Exper. Psychol.*, 11, 1928, 17-26.

Assuming, then, that we have in Fig. 2 a pitch-scale possessing adequate validity, as shown by comparison with other experiments, and reasonable reliability, as revealed by the *Os*' variability, what can we do with it? The remainder of the paper is devoted to this question.

The Subjective Size of Musical Intervals

As was pointed out in connection with the earlier pitch-scale, we can measure the subjective size of various musical intervals by determining from Fig. 2 how much the pitch changes in going from one end of the interval to the other. In general, the subjective size of a musical interval

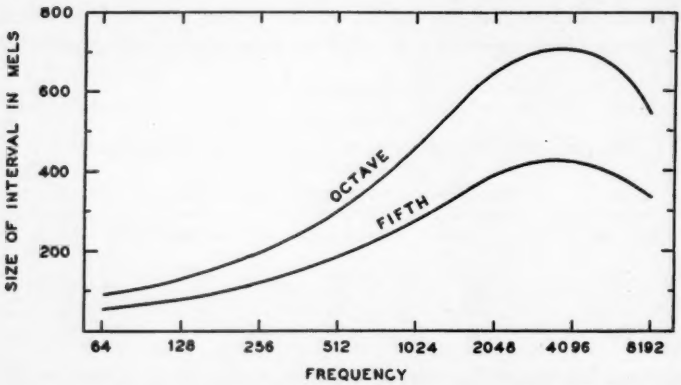

Fig. 6. Pitch-Extent of Musical Intervals

The pitch-extent of musical intervals depends upon where the intervals are located on the frequency-scale. The values on the abscissa represent the mid-points of the intervals whose subjective extents are determined from the ordinates.

is proportional to the slope of the pitch-function at the frequency which falls in the middle of the interval.

In order to illustrate these relations, the subjective size of successive octaves and fifths, as measured in mels, is plotted in Fig. 6. The plot for other intervals would be similar in form but different in ordinate value.

Quite definitely, musical intervals become subjectively larger as frequency increases up to about the fourth octave above middle C. In other words, throughout what is usually called the musical range, intervals made up of equal frequency-ratios increase in perceived pitch-extent with increasing frequency of the stimulus. This conclusion is not entirely novel.

Stumpf declared that, according to his own perception of them, the upper octaves are perceptually larger than the lower octaves.[14] Thus the pitch-scale enables us to confirm and quantify facts apparent to an old master.

Incidentally, as pointed out elsewhere,[15] these facts contradict the wide-spread notion that "equal ratios of frequency give rise to equal intervals of pitch."[16] They also discourage what appears still to be a prevalent belief; namely, that the musical scale is a subjective scale. Woodworth says: "A well developed and much used subjective scale is seen in music; the violinist tunes his four strings to equal intervals by ear, and not by counting the vibrations."[17] That to which the musician actually tunes his strings is a certain ratio of *frequencies,* not *pitches,* because the musical scale is a scale of frequencies. On this scale, "equal interval" means "equal in frequency-ratio," but Fig. 6 shows that equal frequency-ratios do not, in general, produce equal pitch-distances.

Relation of the Pitch-Scale to the Functioning of the Inner Ear

Stevens, Volkmann, and Newman compared their pitch-scale of 1936 with the function relating the frequency of tones to the region stimulated on the basilar membrane.[18] They found reasonably good agreement, but in the case of the revised pitch-scale the agreement is, if anything, even better. The comparison is made as follows.

Experiments designed to determine the location of the region on the basilar membrane which is made to vibrate most vigorously by different frequencies have been conducted by means of the electrical recording of potentials generated in the ears of guinea pigs. As a result of these studies, maps have been made of the basilar membrane locating the region most sensitive to each audible frequency. The data obtained in such an experiment by Stevens, Davis, and Lurie[19] are shown by the hollow rectangles and circles in Fig. 7. The filled circles are taken from Steinberg's[20] report of a 'mapping' experiment conducted by Culler. Now, if we lay the pitch-scale off along the distance (ordinate) repre-

[14] C. Stumpf. *Tonpsychologie,* 1, 1883, 250.
[15] Stevens and Davis, *op. cit.,* 84.
[16] A. H. Davis, *Modern Acoustics,* 1934, 235.
[17] R. S. Woodworth, *Experimental Psychology,* 1938, 394.
[18] Stevens, Volkmann, and Newman, *op. cit.*
[19] S. S. Stevens, H. Davis, and M. H. Lurie, The localization of pitch perception on the basilar membrane, *J. Gen. Psychol.,* 13, 1935, 297-315.
[20] J. C. Steinberg, Positions of stimulation in the cochlea by pure tones, *J. Acoust. Soc. Amer.,* 8, 1937, 176-180.

senting the linear extent of the basilar membrane, we can plot the solid curve shown in Fig. 7, and we find that the functions coincide to a degree well within experimental error. If, as appears probable, the maps of the basilar membrane shown in Fig. 7 approximately represent the human cochlea, we can proceed to some interesting conclusions.

Apparently frequencies separated by equal intervals of pitch stimulate the basilar membrane at equi-distant points. Consequently, when an *O*

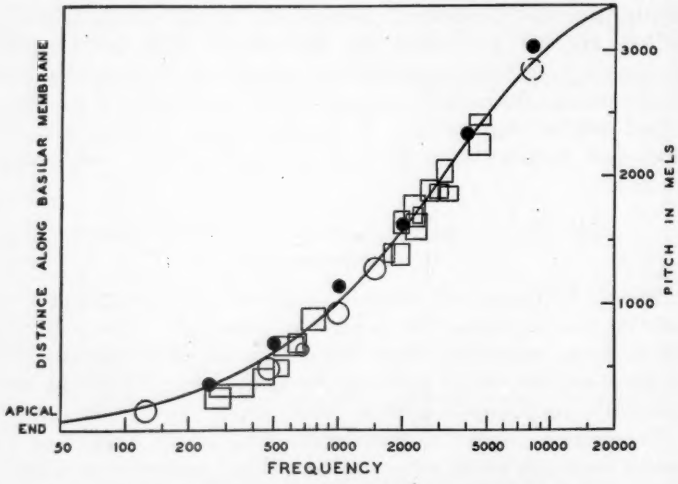

FIG. 7. LOCATION OF PITCH ON THE BASILAR MEMBRANE

Comparison of the pitch-function with data locating the positions on the basilar membrane which are stimumlated by tones of various frequencies. The linear extent of the basilar membrane is represented by the ordinate (left) and along the same axis is laid the pitch-scale (right). The curve represents the pitch-function; the hollow rectangles and circles (Stevens, Davis, and Lurie) and the filled circles (Culler) show the locations in the cochleas of guinea pigs of stimulation by various frequencies.

is asked to adjust one tone until its pitch is half as high as that of another tone, he would be adjusting the frequency until it stimulates a position on the basilar membrane midway between the position stimulated by the higher tone and the apical end of the membrane. He would not, of course, be aware of these locations as such, but the underlying physiological process which makes comparison of pitches possible appears to be characterized chiefly by a spatial differentiation of such a simple sort. This inference accords with the fact that, except for slight differences

at the two ends, the neural innervation of the basilar membrane is of essentially uniform density throughout its length.[21]

Although there are subsequent central nervous processes, it appears then that the form of certain discriminatory functions is imposed by the receptor mechanism.

Relation of the Pitch-Scale to Differential Sensitivity

When Fechner performed the mathematical integration leading to what we call the Weber-Fechner law, one of his assumptions was that just perceptible differences are not only equal in the sense of being equally noticeable, but are subjectively equal as well. Psychologists have long debated the validity of this notion. Equipped with an adequate scale of subjective pitch, we should be able to resolve this question as to whether two frequencies which are just noticeably different are also separated by equal intervals of pitch. Stevens, Volkmann, and Newman[22] made the necessary comparison of the earlier pitch-scale with some of the data on differential sensitivity obtained by Shower and Biddulph[23] and they were able to conclude that, within the experimental error, all jnd's determined at a constant loudness-level were subjectively equal, as measured by the 1936 scale of pitch.

Now, since considerable variety exists among data reported in experiments on differential sensitivity to frequency, we propose here to follow a somewhat novel procedure. We shall not compare the new pitch-scale with a function obtained by summating DL's; instead, we shall first make the assumption that jnd's for pitch are subjectively equal, and then proceed to differentiate the pitch-function (Fig. 2) in order to discover the size of the DL's as predicted by the pitch-scale. In other words, we are going to carry out a reversal of Fechner's procedure: he began with the DL and arrived at a magnitude function for sensation—we begin with a magnitude function and find something out about the DL.

According to the assumption just made, the number of DL's within any small frequency-interval is proportional to the number of mels within the interval. In order to evaluate the constant of proportionality here involved, let us make a second assumption: that between 2000 and 3000 $\sim$ the value of the fraction $\Delta F/F$ is 0.002. All experimenters are

[21] For a plot showing the density of innervation of the basilar membrane, see H. Fletcher, The mechanism of hearing as revealed through experiment on the masking effect of thermal noise, *Proc. Nat. Acad. Sci.*, 24, 1938, 265-274, esp. Fig. 6.

[22] Stevens, Volkmann, and Newman, *op. cit.*

[23] E. G. Shower and R. Biddulph, Differential pitch sensitivity of the ear, *J. Acoust. Soc. Amer.*, 3, 1931, 275-287.

agreed that over this region the fraction is substantially constant and there is adequate evidence that 0.002 is a reasonable value. We can now proceed to compute the number of DL's between 2000 and 3000 ~ (the number is 202), and, since we know the number of mels between these frequencies (about 420 mels, as determined from Fig. 2), we can take the ratio of these two numbers and thereby obtain the constant of proportionality relating DL's to mels. By graphical methods we can now

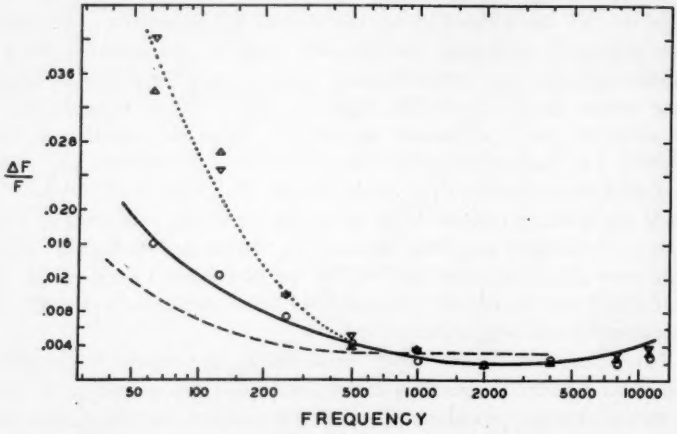

FIG. 8. WEBER'S FRACTION AS COMPUTED FROM THE PITCH-FUNCTION
The solid curve shows the value of Weber's fraction as predicted by a differentiation of the pitch function in Fig. 2. Knudsen's data for measurements of ΔF/F are plotted as a dashed line. The remaining points are for data gotten by Shower and Biddulph: the inverted triangles they obtained at a sensation-level of 40 db., the other triangles at 50 db. The hollow circles stand for data obtained under their method of abrupt variation—the traditional technique for measuring differential sensitivity.

determine from Fig. 2 the number of DL's within any small frequency-range and hence discover the size, in cycles, of DL's at various frequencies. Next we change the DL's into relative DL's, *i.e.* we express the DL as a fraction ΔF/F, and plot the result as shown by the solid curve in Fig. 8.

This solid curve in Fig. 8 is our prediction, made from the pitch-scale, of the function relating Weber's fraction, ΔF/F, to frequency. The agreement of this curve with actual experimental determinations is satisfactory for frequencies above 500 ~, where the experimental results themselves are generally consistent. Below 500 ~, however, different

experimenters, using different experimental techniques, have produced widely divergent data. The curve we have derived from the pitch-scale may or may not aid us eventually in selecting among these data, but certain comparisons are interesting. Knudsen's data are presented in Fig. 8 as a dashed line.[24] Among the lower frequencies, Knudsen obtained smaller values for $\Delta F/F$ than are predicted by the solid curve. Shower and Biddulph criticized Knudsen's technique because it allowed transient frequencies (introduced by the switching on and off of the stimulating tones) to provide the *O*s with unwanted cues. These critics then set out to remedy the situation by means of an apparatus which changed a tone from one frequency to the other in a continuous manner. Their stimulus, therefore, was a frequency-modulated tone.[25] With this stimulus they obtained the data represented by the triangles in Fig. 8.

Shower and Biddulph also made some determinations of $\Delta F/F$ using the more traditional method of abrupt variation, where the switching from the lower to the higher tone is instantaneous rather than gradual. Their results under this method are shown by the circles in Fig. 8. These results accord with the values predicted by the pitch-scale, but, before we can decide upon the validity of this agreement, we shall have to probe more thoroughly the factors influencing differential sensitivity to frequency—especially for the lower tones.

Hypothesis for a General Law of Psychophysics

It will be remembered that the solid curve of Fig. 8 was derived from the assumption that all jnd's for pitch are subjectively equal, *i.e.* all tones separated by a given number of DL's will appear equi-distant in pitch. The situation disclosed by Fig. 8 suggests that, to the extent that present data are able to decide the issue, this assumption is confirmed by experiment. We can consider the jnd's for pitch, at a constant loudness level, to be subjectively equal.

On the other hand, an analogous treatment[26] of the data for the discrimination of intensity shows clearly that jnd's for loudness are *not* subjectively equal. Instead, these jnd's increase rapidly in size as the intensity of the stimulus is raised.

Now, the physiological mechanisms which mediate our discriminations of pitch and loudness are of two basically different types. When the

[24] V. O. Knudsen, The sensibility of the ear to small differences in intensity and frequency, *Phys. Rev.*, 21, 1923, 84-103.

[25] For a discussion of the nature of this stimulus, see Stevens and Davis, *op. cit.*, 89-94.

[26] Stevens and Davis, *op. cit.*, 148-152.

frequency of a tone is raised the pattern of excitation is shifted along the basilar membrane—new excitation is *substituted* for old. Thus we tell one pitch from another because the locus of excitation on the basilar membrane has been altered. When we increase the intensity of a tone, however, the general locus of the excitation in the cochlea remains relatively fixed and new excitation is *added* to old. Thus we detect that one tone is louder than another tone of the same frequency whenever there is an addition of sufficient excitation. Perhaps the subjective size of jnd's is related to the kind of discriminatory mechanism which mediates them.

Considerations of this sort have led to the following hypothesis. All jnd's based upon a physiological process involving a change in the general location of excitation (or possibly a change in its form of distribution) will be found to be subjectively equal in size. All jnd's produced by a process of adding excitation to excitation already present will be found to increase in subjective size with increased magnitude of the stimulus.

Such an hypothesis may eventually find itself refuted.[27] It will almost certainly have to submit to some degree of revision. Corrections will be in order after we have made reliable measurements of differential sensitivity and after we have created adequate subjective scales by methods similar to those of the present experiment.

Summary

(1) Our purpose was to erect a new subjective scale for the measurement of pitch. This scale was to be an *extensive* scale—one to which numerals are assigned in such a way that the positional relations among the numerals in the numeral-series are reflected in perceived relations among the pitches.

(2) We also sought the answer to certain questions arising from previous experimentation: Can the same scale be obtained by different methods, such as fractionation, bisection and equal sense-distances? How much variability is to be expected within the results of each *O* and among the results of different *O*s? How does pitch behave at very low

[27] The hypothesis appears to be confirmed by such meager data as are now available for the discrimination of visual brilliance and saturation. As already pointed out elsewhere (for reference see footnote 2), the data indicate that jnd's for saturation are subjectively equal, whereas the jnd's for brilliance grow in subjective size as the intensity of the stimulus increases. A jnd in brilliance, we might assume, occurs when the total amount of visual excitation is adequately increased; whereas a jnd in saturation is probably mediated by a change in the relative distribution of the excitation among the different types of receptors in the retina.

frequencies? What happens in an experiment on fractionation when *O* is supplied with a reference tone defining zero pitch?

(3) Two experimental methods were employed. (a) Ten *O*s adjusted groups of frequencies in such a way as to divide each of three partially overlapping frequency-ranges into four equal pitch-distances. (b) Twelve *O*s adjusted a second tone until it sounded half as high in pitch as a given standard tone.

(4) The results of these two experiments showed excellent agreement—well within experimental error—and from the data a scale of pitch (Fig. 2) was constructed. The unit of the scale is called a *mel* and is defined as one thousandth of the pitch of a 1000 $\sim$ tone.

(5) This new scale is like, but not identical with that proposed in 1936 by Stevens, Volkmann, and Newman. The differences are due to apparatus and to method. The new scale accords strikingly with data on the bisection of tonal intervals obtained by Pratt, and is in fair agreement with the older data of Lorenz and Münsterberg.

(6) Using the pitch-scale to measure the subjective size of musical intervals, such as octaves and fifths, we find that the pitch-extent corresponding to a given interval varies with the frequency-region in which the interval occurs. Thus octaves and fifths increase in subjective size as we go up the musical scale to about the fourth octave above middle *C*.

(7) Comparison of the new pitch-scale with functions showing the location of stimulation on the basilar membrane demonstrates that frequencies separated by equal intervals of pitch excite the basilar membrane at points of approximate equi-distant separation.

(8) The pitch-scale was used to predict the differential sensitivity of the ear, as a function of frequency. From this analysis we are able to conclude that all jnd's for pitch are essentially equal in subjective size.

(9) Since the jnd's for loudness are not equal in subjective size, and since the mechanism for pitch discrimination differs from that for loudness, the facts of the present experiment support the hypothesis that the subjective size of jnd's depends upon which of two basic mechanisms is involved in their production.

THE CRITERIA USED IN PREFERENTIAL JUDGMENTS OF GEOMETRICAL FORMS

By EDWARD N. BARNHART, Cleveland Museum of Art

Few attempts have been made by psychologists to disclose the nature and manner of operation of the factors determining our reactions to forms although a considerable amount of work has been done on determining preferences for them. The most extensive treatment is that of Fechner,[1] who discussed in 1876 some of the factors which determine our preferences for geometrical shapes, distinguished between 'formal' and 'associative' factors, and carried out experiments on preferences for rectangles.

The formula of the 'esthetic measure' devised by Birkhoff treats only of the formal or geometrical properties of forms.[2] Holding that the esthetic experience in general is determined by the density of ordered relations in the esthetic object, he expressed this mathematically by the equation $M = O/C$, where M is the esthetic value, O the order, and C the complexity. Applying this general formula to various types of esthetic objects, he attempts to specify exactly what O and C are for a series of polygons and derives their 'esthetic measure.'

The validity of the reasoning upon which the formulae are based has been attacked by Davis who found that there was no correlation between the esthetic measure of the forms and the average rank assigned by two groups of *Ss*.[3] Beebe-Center and Pratt also found large individual differences among their *Ss* in evaluating the forms.[4]

Harsh, Beebe-Center and Beebe-Center attempted to isolate some of the "differential factors" or "principles of judgment" by means of factor analysis.[5] Thirty *Ss* ranked 26 forms selected from the entire range of values as given by Birkhoff's formula. Forms which possessed possibilities of meaning, or common associations, such as crosses, stars, and crowns, were eliminated. Four independent common factors were disclosed which were of differing importance in the judgments. Psychologically interpreted they were described by the authors, in approximate order of their importance, as "smoothness of contour" or "complexity of design within a simple

* Accepted for publication September 1, 1939. This experiment was carried out in the Psychological Laboratory of the University of California under the direction of Professor Warner Brown.

[1] G. T. Fechner, *Vorschule der Aesthetik*, 3rd ed., 1925, 100-108; 184-202

[2] G. D. Birkhoff, *Aesthetic Measure*, 1933, Chaps. 1-3.

[3] R. C. Davis, An evaluation and test of Birkhoff's aesthetic measure formula, *J. Gen. Psychol.*, 15, 1936, 231-240.

[4] J. G. Beebe-Center and C. C. Pratt, A test of Birkhoff's aesthetic measure, *ibid.*, 17, 1937, 339-353.

[5] C. M. Harsh, J. G. Beebe-Center, and R. Beebe-Center, Further evidence regarding preferential judgment of polygonal forms, *J. Psychol.*, 7, 1939, 343-350.

outline;" symmetry, with rotational and diagonal more important than horizontal and vertical; simplicity and regularity; and presence of "odd points" or "tall upward reachingness."

It was our purpose in this study to discover what reasons would be given by a group of *Ss* for their preferences for a set of geometrical forms chosen at random, as expressed in a rank order. It was undertaken more

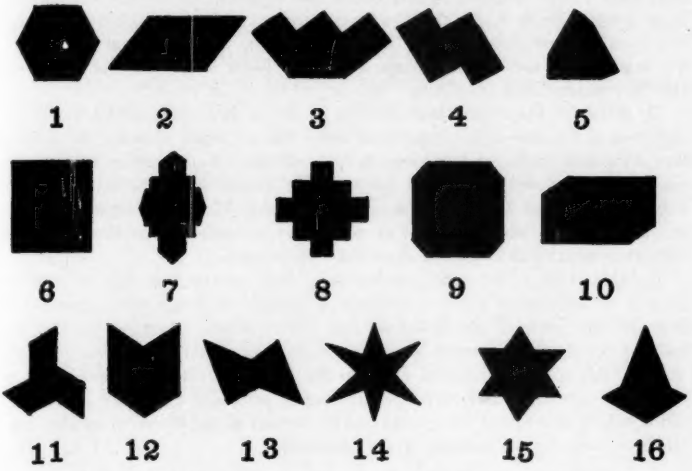

FIG. 1. THE GEOMETRICAL FORMS USED AS STIMULUS-FIGURES

as an exploration to discover what types of considerations, either arising from the actual characteristics of the shapes or on their 'meaning' for the *Ss*, would be found and what information about them could be secured from the *Ss*' verbal statements than to measure quantitatively their importance or analyze their psychological nature.

Since this type of experimental analysis is based on the *Ss*' remarks, it has obvious limitations since it depends on the extent to which the *Ss* were aware of the factors operating to bring about their judgments and in their ability to analyze and describe them. Nevertheless it seemed desirable to ascertain what principles of judgment would be found and to gain some idea of their relative importance.

MATERIALS AND INSTRUCTIONS

The *Ss* were 50 women; sophomores or juniors and members of an introductory class in psychology. The stimulus-figures, 16 geometrical forms, were cut out of dull blue paper and mounted on white cardboard rectangles 3 x 4 in. in size. These

forms were selected from the list of 90 analyzed by Birkhoff and given an 'esthetic measure.' They are illustrated in Fig 1, with their number as used in this experiment.

These forms were presented to the *Ss* individually with the request that they be arranged in rank-order according to preference. When this was completed, *E* asked *S* to give the reasons for the judgments, allowing *S* complete freedom of expression and order of treatment. If some reason was not clear, *E* asked a question or two, but no attempt was made to push the *S* into definitions or esthetic theory, and if a clear reply was not forthcoming the matter was dropped. An attempt was made in every case to treat the matter easily and not to make it so important that the *S* would feel forced into rationalizations or "conceptualizations." When this was completed *S* was requested to successively arrange these same forms in the order of their complexity, symmetry, and familiarity.

To determine the esthetic sophistication of the *Ss* they were asked a number of questions at the end of the experiment about the art work, drawing, or painting they were then taking or had taken in high school, the appreciation courses they had had or were then taking, the household art courses taken, the extent of their interest in visual art, as shown by museum attendance, leisure-time artistic activities, or other behavior, and the extent to which they remembered any theories taught them as to what made a figure or shape beautiful or ugly.

Eight *Ss*, 16% of the group, indicated by their answers that they were at the time of the experiment more or less actively engaged in design work, generally in household art courses.[6] The remainder had had no actual art courses or very few and did not show any interest in visual arts. In general, then, we were apparently dealing with an unsophisticated group to the extent that they were not overly interested in visual art, had very little training or particular experience with it and did not show evidence of being motivated by theories of proportion or esthetic principles relevant for the material of the experiment.

Results

Preferences. Table I gives the group order of preferences for the forms. The specimens are ranked according to their median rank values. Their 'esthetic measure' as given by Birkhoff is also included.

It will be seen from this table that the *Ss* did not agree very well in their relative evaluations of the stimulus-figures. Five of them were placed in every rank position from first to last place. Almost every one was liked most by some *Ss* and disliked most or was placed low in order by others. A rough group-order is, however, discernible.

---

[6] Of this group of *Ss* only 3 stated that they remembered theories of beauty taught to them in school which might have influenced their judgments. They were on the whole, however, quite vague. One *S* had some recollection that slenderness and gracefulness were always beautiful. Two of them remembered a little about the principles of composition and proportion but were unable to be specific when questioned. One *S* stated that she remembered a great deal, which was an over-estimation, for it turned out that she only remembered being taught that "plain forms were not as interesting as complicated and fancy ones, especially those that looked different." Her rank order did not follow this principle.

Despite the lack of agreement among the *Ss* some of the stimulus-figures are evaluated fairly uniformly. No. 5, for instance, is clearly disliked by the group, though some rank it as high as fourth. Nos. 14 and 7 were rather uniformly liked, though placed in last place by some of the *Ss*.

Examination of the individual arrays showed that certain of them departed considerably from the consensus of opinion and were quite different from the group order. Eight such orders stood out as being greatly at variance with the judgment of the other *Ss*.

Bases of judgments. A survey of the reasons or 'criteria' advanced by

TABLE I

RANK-ORDER OF STIMULUS-FIGURES JUDGED FOR PREFERENCE, WITH MEDIAN RANK, STANDARD DEVIATION, RANGE, AND BIRKHOFF'S VALUE OF ESTHETIC MEASURE

Rank-Order	Stim.-Fig.	Med. Rank	S.D.	Range	Esth. Meas.
1	14	.4	3.4	1–16	.33
2	7	.7	3.9	1–16	.25
3	8	3.4	3.5	1–15	.50
4	15	3.8	3.7	1–15	1.00
5	16	4.7	4.4	1–16	.00
6	1	6.4	2.8	1–14	1.00
7	12	8.3	2.8	2–15	.17
8	9	8.6	3.6	2–16	.50
9	10	9.0	3.4	2–15	.33
10	4	9.4	3.7	1–15	.29
11	2	9.7	3.2	2–16	.50
12	3	9.8	3.8	1–16	.11
13	13	10.4	3.4	3–16	.00
14	6	10.6	3.5	1–16	1.50
15	11	12.0	3.9	2–16	−.11
16	5	15.1	2.7	4–16	.00

the *Ss* for their preferences reveals a wide variety. Some were apparently interested in the properties of the figures, in such qualities as symmetry or asymmetry, complexity or simplicity. Connotative, or associative factors were also present, as were statements indicating the *S*'s recognition or lack of recognition of the shape and her past experience with it. To other *Ss*, the potentialities of the form for use in large designs, as on textiles or tiles, served to determine its rank-order.[7]

'Formal criteria' are those which refer to the actual properties and characteristics of the form, either specific parts or the general appearance

[7] As might be expected, many statements were made, presumably as reasons, which were indefinite and which the *S* could not elucidate. Among these were statements expressing reasons which apparently referred to the characteristics of the forms, such as "clear-cut" and "completeness of figure," while some apparently were more like evaluative comments, "interesting" or "nice shape." Three *Ss* could give no clear reasons for their judgments.

and basis of construction. 'Connotative criteria' are those which are based on the *Ss* experience with the form in the past, either a specific association or a recognition of the forms familiarity or novelty. 'Design potentiality' refers to the considerations by the *Ss* of the possibilities of practical application of the form in textiles, tiles, or in non-specified contexts.

In Table II is presented a list of the reasons for judgments which were understandable, the frequency with which they were mentioned by the

TABLE II

TYPES OF CRITERIA AND FREQUENCY OF OCCURRENCE

	No. *Ss* mentioning	Frequency with which only criteria given
(1) Formal criteria		
(a) Part interests		
Angles	7	1
Lines	4	
(b) Whole interests		
Symmetry	16	1
Complexity	14	2
Simplicity	7	
Regularity of construction	4	1
Irregularity	1	
Definite pattern or form	9	1
Organization; 'good design'	2	
Rhythm of construction	2	1
(2) Connotative criteria		
(a) Specific associations	13	1
(b) Recognition		
Familiarity	3	
Unfamiliarity	11	1
(3) Design potentiality	8	
		Total 9 (18%)

group of 50 *Ss*, and the number of times each of these criteria were the sole basis of a *S*'s judgments.

It is apparent that, for this group of *Ss* and these forms, those interests which we classify as formal were most important; and of these those concerned with the appearance or structure of the form are of greatest significance. Symmetry of construction and complexity-simplicity were frequently given formal criteria.

Table III gives the frequency with which judgments were based on one type of criterion or on several, and the combinations that were found. Nine *Ss* (18% of our group) held to one criterion for all their judgments, counting the *S* who judged on the basis of the specific associations evoked. Almost half of the *Ss* (46%) based their judgments on one *type* of criterion only.

It is apparent that evaluative judgments for this type of material are likely to be based on a number of different *types* of criteria or on a number of different criteria of the same general type, as with the many formal interests. The judgment of a whole series of forms on the basis of the same demand or interest is not the usual thing.

Formal criteria. The formal interests fall into two fairly distinct types:

TABLE III

FREQUENCY WITH WHICH PREFERENCES WERE MADE ON BASIS OF ONE OR MORE TYPES OF CRITERIA AND THEIR COMBINATIONS

One type of criterion only			
Formal			
1 specific criterion only	7		
2 or more specific criteria	13		
	20	20	
Connotative			
Specific association	1		
Unfamiliarity	1		
	2	2	
		22	44%
Two types of criteria			
Formal and specific association		8	
Formal and familiarity	2		
Formal and unfamiliarity	5		
	7	7	
Formal and design potentiality		2	
		17	34%
Three types of criteria			
Formal, specific association and familiarity		1	
Formal, specific association and unfamiliarity		1	
Formal, specific association and design potentiality		3	
Formal, design potentiality and unfamiliarity		3	
		8	16%
No criteria		3	6%
Total		50	100%

those concerning a specific quality or property of *parts* of the figure, or presence in the shape of a valued characteristic, such as the presence of angles, lines of various kinds, and proportions, and those concerned with the appearance or characteristics of the *whole* shape or its "structural relationships," such as symmetrical construction and complexity.

(a) Part characteristics. A small number of *Ss* (7 or 14%) mentioned the specific characteristic of the angles on the forms as important in determining their preference for the forms.

Three *Ss*, *C*, *J*, and *L*, stated that they liked forms with "angles" protruding. *C* qualified this by saying that there must not be too many of them. Four subjects described them as "points." For one *S*, *J*, the specific characteristic of "protruding points" was the sole criterion for judgment. Forms were liked with "many slender" points. Her order of preference was No. 14, 16, 15, 11, etc.

A few *Ss* mentioned line characteristics as important considerations.

G stated that there must be a "harmony of lines." On further questioning this turned out to be close to "opposing lines in the shape being parallel or equal." This was similar to the criterion of No. 12 who liked forms with strongly present "parallel lines." No. 9 liked "even lines," or long unbroken lines bounding the form. No. 19 preferred long "diagonal" lines in the forms.

For no *S* was this interest in lines the sole criteria of preference.

(b) Whole-form characteristics. The majority of the *Ss* mentioned as important considerations the appearance of the whole form, its structure, its complexity or simplicity or other characteristics.

(c) Symmetry. Symmetry of one type or another was mentioned by 16 *Ss*, or about a third of the group, as a reason for preference.

(d) Complexity. Complexity was given by 14 *Ss* (28%) as a reason for preference. Contrariwise, 7 *Ss* stated that the simplicity was a desirable characteristic. As one *S* mentioned both as leading to preference, a total of 20 *Ss* (40%) mentioned one or another of these characteristics.

As with symmetry, various views on what is meant by complexity will be discussed later.

The regularity or irregularity of the figure was a fairly important consideration. Four *Ss* (8%) preferred what they termed "regular" figures, while one *S* preferred those termed "irregular."

Dl, holding to the criterion of irregularity stated that irregular forms were "more alive and interesting" and were preferable to the "static forms" which were regular. This quality she felt came from the parts "shooting out in odd directions." Her first eight forms were interesting from this standpoint while the rest were not "good" as they were "regular and uninteresting." Her order was No. 11, 4, 3, 16, 14, 5, 13, etc.

Of the 4 *Ss* mentioning regularity, *Bd* felt that it was "being the same on all sides." She followed this criterion fairly closely (order: No. 1, 6, 8, and 9). She expressed a dislike for stars and a preference for square-like figures over those with long arms or points. *Cb* felt that regularity consisted in the figure having "an equal number of units on each side of the center, horizontal and vertical" which was the same definition approximately that she gave for symmetry. A regular figure was then one in which the components were arranged around a center in a recognizably clear manner. This arrangement could be complex, in fact she mentioned a preference for complexity and her order among the forms which were regular was from the most complex to the simplest; her order began No. 7, 14, 8, and 15. *Cd* also stated that regularity was equivalent to symmetry and consisted in being "even all around." *Ck* felt that regularity consisted in the figure being divisible into units that were "regular" that is, the common geometrical shapes, squares, rectangles, and triangles.

Apparently closely allied to this interest in definite pattern was the criterion of "organization" which, however, remained unclear, and was in two cases synonymous with "good design."

Cb used the word and stated that it means "the way in which the form is put together." *N* mentioned "organization," and "good design," but could not do more

than point out those forms that possessed it. Both their orders began with No. 7, 8, and 14.

Rhythm was mentioned by 2 *Ss* and was apparently an almost ineffable quality.

For one *S (Ck)*, it constituted the sole criterion. To her it meant something akin to "movement," a property of the whole form, which made it "alive." It apparently had to do with the arrangement of protruding angles and the directions of the lines. Her order began No. 11, 13, 4, and 5 and ended with No. 16, 7, 8. *Q* mentioned it as important but could not define it. Her order began No. 16, 12, 2, 11. She thus differed from *Ck*.

Definite pattern or shape was the criterion advanced by 8 *Ss* and formed the sole judgmental basis of a ninth. In most cases this depended on the analysis of the form into elements and concerned their pattern of assembly. In a few cases it seemed to refer to the fact that the form could be seen as built upon a fundamental background or net-work of lines, either horizontal-vertical or diagonal criss-crossing.

C stated that this meant a "definite pattern of arrangement." The basis on which the form was built must be clear, that is, the constituent elements must have a "regular" or orderly manner of assembly. Her order began: No. 8, 7, 14, and 4. *D*'s view was similar to this, as was that of *H* who stated that the "parts (must be) fairly evenly distributed in a definite relation to one another." *N* expressed a dislike for forms that were "a bunch of lines drawn any which way, no purpose in them" and demanded forms with a "definite pattern" which she explained consisted in a form having a "unity of lines, all parallel to each other." *Bg* also visualized the form's constituents, "little elements." The form was liked when the manner in which it was put together was "clear." *Db* stated that a form must have a "plan" on which it was built. *Cj* stated that there must be some "reason" to the manner in which the form was made.

The general appearance of the form, its structure and the relations of its parts were thus important considerations in the judgments of the *Ss*. In all 41 *Ss* (82%) mentioned "whole" factors and 10 *Ss* (20%) mentioned "part" factors. The complexity of the shape and its symmetry were the two most frequently mentioned characteristics leading to favorable judgment; complexity was mentioned by 14 *Ss* and symmetry by 16.

The criteria advanced by the 8 *Ss* whose arrays we have distinguished as being quite different from the consensus order of the group shows that six of them did not advance different formal bases than the majority did, and that none of the irregularities are to be explained on the basis of associations. From their statements one would not conjecture that their orders would differ from those of the other *Ss* giving the same reasons. Either from a misuse of the criteria or from an unusual manner of seeing a criterion exemplified in the forms, they gave, however, the same reasons as many other *Ss* and yet ordered the forms differently.

The 2 *Ss (Ck* and *Dl)* whose criteria as well as preference orders did differ held respectively to criteria of 'rhythm,' *(Ck)* and irregularity and simplicity *(Dl)*.

Connotative criteria. We classify as "connotative" those considerations which were given as bases for preference and which were not concerned with the specific characteristics of a form but arose from the *S*'s previous experience with the form and her recognition of this. We distinguish

here two types: specific associations and a feeling of familiarity or unfamiliarity.

(a) Specific associations. For some *Ss* associations did not play a rôle while for others it was a decisive factor. Table IV gives the frequency with which the *Ss* indicated the presence and influence of associations of this type.

Half of the *Ss*, therefore, reported, after they had arranged the forms and given their criteria, that the forms had not suggested anything to them or had caused them to recall an experience. About one-fifth, *i.e.* 11 *Ss* had had some associations but stated that they did not believe these were important in their judgments. One of these *Ss* stated that every one of the shapes had aroused some association but that none of these were important in judgment.

Of the 13 *Ss* in whose judgments the associations which the forms aroused were definitely influential in judgment they were the decisive factor for one *S*, for 6 they

TABLE IV

FREQUENCY OF OCCURRENCE OF ASSOCIATIONS AND INFLUENCE ON JUDGMENTS

	Ss	
	No.	%
(A) Associations not determining judgment		
(1) No association at all	24	48
(2) Association after judging	2	4
(3) Associations present but not considered	11	22
	37	74
(B) Associations influencing judgment	13	26
Total	50	100

were joint factors or of secondary importance, and for 6 they were of small importance but nevertheless present.

Bf stated that her preferences were determined throughout by the object which the form suggested. If the reference was pleasant, the form was liked. Forms which had no associations or to unpleasant things were disliked. Her order was close to the group order: No. 4, 8, 15, 7, etc. The stars were preferred because "stars" were "pleasant" and "beautiful;" form No. 8 resembled a "jewel-box;" No. 7 a cut gem, and so forth.

For 6 *Ss* specific associations played a rôle together with some other factor, such as an interest in formal characteristics.

J divided the forms into two groups: those having "good" associations and which were symmetrical, and those having "bad" associations and which were unsymmetrical or irregular; the former were preferred. The remaining forms ranging in between these groups were "indifferent." Four forms were in the first group: No. 3 a crown; 14 pin-wheel; 7 snowflake. In the disliked series of five forms a "bad" association was the "Red Cross" to form No. 8.

L associations of "evening star" No. 14; gem 9, stove 16, and a box 10 led to these being liked; her other criteria were presence of angles and symmetry. There were no associations with forms at the disliked end.

Bk stated that all the forms had "meaning" to her, which arose immediately. The more meaning they had, the more they were associated with things and objects she remembered, the more they tended to be liked; those at the disliked end of the order

had "less" meaning. She estimated such associations as forming one-half the basis of her judgment; the other half was "good design." *Cd* had unpleasant associations with forms which reminded her of geometry and physics courses and such forms she placed at the disliked end. *Ck* had pleasant associations with the first six forms in her order: No. 14 and 16 stars; 12 an open book; 10 a box; 3 a bowl, and 16 a peaked hat. She stated that these associations were important in determining her preferences. *Dh* said that probably one basis for judgment was the suggestions of Indian designs. Her other criterion was formal, a liking for "ornate and definite shaped figures."

Associations were minor factors in the judgments of 6 other *S*s. We find here that they specifically influenced the judgment of only one shape or were of a "general" influence for 5 *S*s which was quite similar to the general feeling of familiarity.

P had only one association, to No. 7, which suggested a diamond ring which was quite pleasant since she had become engaged shortly before the experiment. This was placed first in the series; her other interests were formal.

For 5 *S*s (*U, V, Bd, Cg,* and *Dk*), a fairly general association was evoked by a number of forms and determined their rank as a group. For *U, V,* and *Bd,* geometry in high school caused a number of shapes to be disliked. *Cg* was favorably reminded of some designs she had once made, and *Dk* had a generalized recollection of paper-cutting in grammar school which was a pleasant recollection and led to the preference for four or five forms. Shape was determinative, however, and she expressed formal interest in "points" and symmetry.

Of the 8 *S*s with a background of art, 3 were influenced by associations—relatively the same number as for the non-art group. As we mentioned above, none of these 8 *S*s with irregular arrays reported that associations were decisive or even important.

(b) Familiarity. We have classified as 'specific associations' those specific and definite recollections evoked by the forms. When *S* was unable to recall particular experiences, when the referent of the association was not clear, or the form did not actually refer to an object but the subject states that she had seen the form before and was familiar with it, we classify these statements under the heading of 'familiarity'—as a separate type of connotative factor.

Fourteen *S*s (28%) explicitly stated that the familiarity or unfamiliarity of the forms were important; 11 *S*s liked forms because they were "unusual," "different," or "new to me," while 3 liked those which had been "seen before," or were familiar.

Among the 11 *S*s who were influenced by the novelty of the shapes, only one, T, stated that novelty was the chief criterion. This *S* arranged the forms in the order of their "unusualness," with no formal interests of importance. For the other *S*s, this was a secondary factor.

In the judgments of the 3 *S*s (*X, Bb,* and *Cg*) who liked familiar shapes, this was a secondary factor and operated in conjunction with formal interests.

Design potentiality. One of the reasons advanced by some of the *S*s for preferences was the possibility of using the various shapes for patterns or designs. These were usually visualized as larger in size and forming an all-over pattern with other identical forms. The degree to which this pattern was attractive and the extent to which the form lent itself to such designing, determined the degree of preference.

No such considerations were present in the remarks of 31 *S*s (63%). Some in-

terest was indicated by 11 *Ss* but this did not, according to them influence their judgments, and remained as a general consideration.

Eight *Ss* (16%) mentioned this factor as an important consideration in their judgments.

D saw the forms as tiles and imagined them in an all-over tile pattern. *G* stated that she "expanded the forms and considered the result as a pattern." *Q* saw them in material as all-over patterns on a dress or textiles; *Y* as the basis for quilt design. *Bd* did not mention the application of the imaginary design. *Cf* saw each form as an element in a larger design, much larger and greatly multiplied, and the relations

TABLE V

RANK ORDER OF SPECIMENS JUDGED FOR COMPLEXITY, SYMMETRY, AND FAMILIARITY WITH MEDIAN RANK AND DEVIATION

Rank order	Complexity			Symmetry				Familiarity		
	Spec.	Med. rank	S.D.	Spec.	Med. rank	S.D.	Freq.*	Spec.	Med. rank	S.D.
1	7	1.1	.7	6	1.1	3.6	11	6	1.1	1.6
2	8	1.9	2.0	8	2.6	2.8	12	15	4.0	2.3
3	14	2.7	1.8	14	2.4	3.6	11	10	3.6	2.9
4	15	3.8	2.1	1	4.6	2.2	11	1	4.0	1.9
5	16	4.9	2.0	9	4.7	2.5	10	2	4.0	2.5
6	3	5.2	2.4	7	4.7	4.1	10	14	4.4	2.6
7	11	5.3	2.3	15	5.5	3.4	11	9	5.3	1.9
8	4	7.3	2.2	2	7.4	3.7	6	12	8.5	2.8
9	13	8.8	1.7	10	8.6	3.2	4	8	8.5	3.0
10	5	9.3	3.3	12	9.0	2.6	8	4	10.8	2.6
11	12	9.9	2.0	16	10.3	3.5	6	7	11.0	2.5
12	1	11.7	1.7	4	10.8	3.1	4	16	11.9	2.8
13	10	12.9	1.5	3	12.0	2.8	7	3	12.3	2.0
14	9	13.00	1.2	11	13.2	2.8	1	13	12.8	2.3
15	2	14.1	1.1	13	13.7	1.6	0	11	13.6	2.3
16	6	15.5	.3	5	15.4	1.1	0	5	15.2	2.6

* Frequency with which form was adjudged symmetrical by *Ss* not ranking items but grouping them.

which each form bore to its identical neighbors determined its preference; a good pattern led to the form being placed high in the order.

Despite the importance of this consideration in the judgments of these *Ss*, it did not form the sole criterion for any. This interest did not lead to any preference orders departing from the group array. Of the group of 8 *Ss* influenced by the design potentiality, 3 were engaged in design work at the time.

Judgments of complexity and its basis. Before summarizing these results, we turn to the results of the symmetry and complexity investigations. When the preference judgments had been completed and the *Ss* asked about their reason for their judgments, they were each asked to arrange the forms in the order of their complexity, and then they were asked to state what their basis for the judgment was.

Table V gives the group order for these judgments according to their

median rank value. There was more agreement here than there was in preference.

Examination of the criteria of complexity reveals three major bases, more or less distinct, two dealing with objective characteristics, one with considerations of construction, and an infrequent basis, familiarity. Table VI indicates the frequencies with which they were mentioned. One third of the group mentioned two or more bases.

The character of the form's outline was the more frequently given basis of complexity. Of this group, 19 *Ss* mentioned angles protruding or "points" as the characteristics chiefly responsible for a complex appearance.

Typical statements were: *A*, "cut-out entrances are complex;" *X* "Lots of angles and points make it complicated;" *Cc* "It's the number of angles and the sharper the

TABLE VI

FREQUENCY WITH WHICH VARIOUS BASES OF COMPLEXITY WERE GIVEN

	Mentioning	Sole basis	
	No. Ss	No. Ss	%
(1) Form outline	31	16	32
(2) Construction difficulty	21	8	16
(3) Element analysis	12	6	12
(4) Familiarity	3	1	2
Total giving 1 basis only		31	62
Total giving 2 or more bases		19	38
Total		50	100

more complex;" and *Cj* "The more angles, the more complex looking; inward angles are important too."

Fourteen *Ss* stated that the number of lines making up the outline was the chief consideration. Some of these felt that the sheer quantity of lines was important. Others stated that short lines gave a form the appearance of complexity.

Bd stated that "more lines and a great number of sides" made a form complex appearing. *D* stated that complex forms were those having lines "going in different ways." *Da* held that "little lines, broken up outline, make it that way" (*i.e.* complex appearing).

Eight *Ss* indicated that the sheer number of "parts," lines, or angles, in the outline was the chief factor.

Typical statements were: *Bb*, "many angles and lines;" *Fl*, "great variety of corners, angles and lines."

The second most frequently mentioned basis for complexity was the estimation of the difficulty that would be met in constructing the forms by others or by *S* herself.

For *A* it was "the idea of the trouble taken to make it oneself," and *F* the "considering the effort to make them." *D* ranked the forms on the basis of her estimation of her difficulty in drawing them, the easiest to draw were the simplest. To *Cb* the most complex were those that "needed more care in drawing."

Three *S*s differed from the majority opinions in stating that their basis for complexity lay in the degree of familiarity the form had for them.

U gave as her basis for judgment for the whole series the degree of familiarity; the complex were those which were completely new to her. Her order was close to the groups. *Da* stated that "simple lines and a familiar shape" made for simplicity and a lack of associations or meanings led to complexity. For *Dc* those forms having the "most lines" were complex; those that were unusual and unfamiliar were highly complex.

Fourteen *S*s gave complexity as a criteria for preference. The reasons advanced by this group were representative of those advanced by those not giving this reason, all four types were present. The *S*s with a background of art also gave all types just as the non-art group did.

Another group of *S*s visualized the forms as divided up into constituent elements of different shapes. The behavior of this group was remarkably similar in that the visualized constituents were simple geometrical shapes, squares, rectangles and triangles, and generally the forms were seen as subdivided in much the same manner.

G partitioned form No. 7 into a central square with a little square on each side, each of these being capped with a triangle. No. 8 was seen divided in the same manner without the triangle, of course. No. 7 was ranked higher in complexity since it had a greater variety of elements, triangles in addition to squares. *L* subdivided No. 7 in the same manner as did *G*, but complexity was determined to her by the "number of parts I can see." She subdivided the other forms similarly into the common type of shapes. *Bf* was the more consistent and prolific form analyzer. All were subdivided carefully, and the more elements involved, quantitatively, the more complex or a greater variety. All were reduced to squares and triangles, or other shapes if necessary.

We find, therefore, that there is some disagreement among *S*s in their estimation of the relative complexity of a group of geometrical forms, and that a number of fairly distinct criteria were advanced as the bases of complexity. Allowing for the fact that verbal description is inexact, it seems likely that several different considerations regarding complexity were present, leading to differences in estimates of degree of complexity, and hence to lack of agreement. Moreover, we found that the group mentioned two or more bases as considerations in their ordering. The extent to which agreement would be found among the *S*s holding the *same* criteria is a mater for further experiment. This consideration holds also for judgments of preference, symmetry, and others.

Judgments of symmetry and its basis. The second request of the *S*s was to arrange the forms in the order of their symmetry, or to indicate which ones were symmetrical and which were not. Four *S*s were unable to do this. They either did not know what symmetry meant, or, if they said that they did have some conception, they were unable empirically to indicate its presence or absence. Twelve *S*s grouped the forms into two or three groups: symmetrical, non-symmetrical, and slightly symmetrical. The consensus of opinion is given in Table V.

As we have mentioned above, the aim of this inquiry was to ascertain what *S* considered symmetry to be—or what she meant when she used the term. A number of bases were advanced which, with their frequency of occurrence in this group, are given in Table VII. It was not often that the *Ss* would use terms such as "horizontal" or "vertical symmetry" or even axis, but in general their meaning was clear.

The most frequent view held was that symmetry arose from the opposition of "like parts" across an axis. Nineteen *Ss* (38%) mentioned this type and stated that the axis must be the vertical one. Opposing "parts" could refer either to parts of the figure, such as arms of stars, or entrant angles.

TABLE VII

FREQUENCY WITH WHICH VARIOUS BASES OF SYMMETRY OCCURRED

	Frequency	
	No. *Ss*	%
(1) Relation of parts to linear axis		
(a) Opposition of like parts; vertical axis	19	38
(b) Opposition of like parts; vertical or horizontal axes or both	8	16
(c) Opposition of like parts; any axis	4	8
Total	31	62
(2) Relation of parts to center		
(a) Relation of analyzed elements to center	2	4
(b) Distribution of like parts around center (radial symmetry)	2	4
Total	4	8
(3) Similarity of form to square	6	12
(4) Rhythmic coördination of parts	1	2
Total no. of *Ss* advancing basis	42	84
Total no. judging on "feeling"	4	8
Total no. unable to judge	4	8
Total	50	100

Typical statements are: *C*, "The figure must be the same on both sides, up and down;" *N*, "The opposite parts must be exactly the same across the center line;" *V*, "Same parts on each side, that is, if an angle sticks out on one side, it must on the other too;" *Cd*, "There must be a balancing of parts of the shape;" *Da*, "If the form was divided into two parts down the center, they would be alike."

Eight *Ss* holding this view stated that the opposition could be across a horizontal axis also. This view was never given independently but always in connection with the view of symmetry as based on vertical-axis. Several of these *Ss* felt that vertical symmetry was a "higher" type of symmetry than the horizontal and some felt that when the two were combined in a form a very high degree of symmetry was achieved.

Among these statements are those of *Bb*, "Balance of parts across either the vertical or horizontal axis but more symmetrical when the sides are alike than when top and bottom;" and *Z*, "Very symmetrical when same in two ways (*i.e.* both

horizontal and vertical symmetry) but still symmetrical if just so across the middle line (*i.e.* horizontal)."

Some *Ss* felt that the horizontal and vertical axes were not the only possible bases for symmetrical arrangements. Three *Ss* stated that other axes were equally "logical," to use the term of *Q*, who stated that symmetry meant the "division of a form along a line, an axis; being equal on both sides. The axis may go in any direction, even various kinds of diagonals."

Y stated that a form was symmetrical when the parts were arranged equally about an axis, generally horizontal or vertical, "but not always necessarily so." *Cj* stated that symmetry meant the opposing of similar parts and like parts" and that the dividing line "follows the natural outline of the figure" (No. 4 to her was symmetrical).

All in all these views were held by 62% of the group. The remaining three types were in a minority.

The dividing-up of the forms was present in the consideration of complexity and 2 *Ss* who held to this view of complexity mentioned it also as the basis for a form's symmetry.

Bk stated that the highest degree of symmetry arose from forms that could be subdivided into equal parts that were related together in a definite way around a point; as she said, "the balance of parts around a center, not radiating, but same parts across the center." She listed forms No. 6, 8, 9, 2, 1, 7, 4, 12, and 10 as possessing high degree of symmetry, in that order, since they possessed "symmetry of line, equal parts, and especially were divisible." (It will be noticed that several of these are not radially organized and her basis is actually divisibility along an axis.) Forms 13, 14, 15, and 3 had less symmetry but "symmetry of outline" and were not divisible. *Bf*, whose views on complexity we have given above, felt symmetry to be a type of radial arrangement of parts about a center. She divided each figure into components; if the form was composed of similar parts it was symmetrical. She also considered the points at the protruding angles: all of them must be the same distance from the center. Those forms with vertical or horizontal symmetry were the second group.

Two *Ss* held to a radial view of symmetry without analysis of the forms into elements.

J said that she "felt lines running from the center outward. If the figure were turned it would be the same." *V* stated that the basis of symmetry was "all the angles (being) equal on the same cross lines." She felt lines through the center to the protruding angles and a symmetrical form was one which had equal angles opposite in this manner.

Six *Ss* felt symmetry to be a product not of opposition of similar parts with reference to an axis, but of the similarity of the figure to a square shape. If the figure could fit neatly into a square outline it was symmetrical. There was also a feeling that simpler forms of square-shape were more symmetrical than more complex ones.

P expressed this clearly; her basis for judgment was "even sides, matching in length" and an explicit statement of the consideration of the extent to which the form was similar to a square: four equal and similar sides, and four corners. *Bg* said that forms that "approached to a square shape were the most symmetrical." And *Db* stated that "plain four-sided ones" were more symmetrical.

One subject, *Ca*, advanced the unique view that symmetry arose from the "rhythm"

of the form, the "coördination" shown in it. She could not explain her views more closely; her order of symmetry began with forms 11, 16, and 4. Four *Ss* could not give objective bases for symmetry but stated that they "just went by the feeling" or the "appearance" of symmetry in the form.

Cl said that it was "just the way in which places and points are arranged." *W* had "no specific theory, just a feeling for it." I went by the "appearance only," and *B* stated that symmetry was "not necessarily bound to an axis" and that she "just went by a feeling for it."

Every *S*, with the exception of two, held to only one view of symmetry. These two added other elements to one or another of the types explained above. *C*, holding to a view of vertical symmetry, added that a "feeling of weight, larger at the bottom and smaller at the top" was important. This is equivalent to 'balance;' her form order does not show that this was followed in her ranking. *Cg* added the factor of simplicity, recognizing the vertical as the basis of arrangement. Among the forms that she arranged on the vertical symmetry basis, she awarded highest positions to the simpler forms: No. 6, the square, was first, followed by 2, 10, and 1.

It is apparent that symmetry meant a number of different things to the individuals in this group, undoubtedly one reason for the only fair degree of agreement in their estimates. The major factor was the axial basis of construction with the vertical axis most important. A radial type in which the analyzed parts of the form were seen as distributed around the center point was apparently a minor consideration. The third type, classified on the basis of statements that square-like forms were symmetrical, might be another way of expressing this view except for the fact that simpler forms were generally held by this group to be the more symmetrical, which is not the case with the majority of *Ss*, as can be seen by the group order, where No. 7, 8, and 14, high in the complexity order, were adjudged more symmetrical than simpler forms on the same basis.

Estimates of familiarity. The *Ss* were requested to arrange the forms in the order of their familiarity with them. This order is given in Table V and reveals a fair degree of agreement in judgment.

Conclusions

(1) The reasons advanced by a group of *Ss* as their bases for preference judgments of a group of 16 geometrical forms revealed three main interests: the formal characteristics of the forms, the connotative or associative meanings, and their potentialities for designs.

(2) One or another formal criteria, those concerned with the structure and appearance of the form, were given by 90% of the group. The aspect of the whole form was much more important than the nature or presence of different parts.

(3) Connotative factors were of two types: specific associations which the forms evoked, and generalized recognitions of the familiarity of the forms, or their novelty. Specific associations were important in determining the judgments of approximately a quarter (26%) of the *Ss*. Familiarity was important for 3 *Ss* (6%) and unfamiliarity for 11 (22%).

(4) Design-potentiality was influential in the judgments of 8 *Ss* (16%). This interest lay in the possibilities of the forms for use in textiles or other all-over pattern uses.

(5) Approximately three-quarters of the group (78%) gave two or more criteria as their bases for judgments. Formal criteria of various types were the most frequent and most important. Connotative factors, specific associations, or recognition of familiarity or unfamiliarity determined some judgments in addition to formal criteria in the orders of one-third (34%) of the *Ss*.

(6) Analysis of the judgments and bases advanced by the *Ss* for complexity revealed a number of dissimilar views. Two-thirds of the group felt it to consist in the type of outline and a multiplicity of angles or lines; difficulty of construction was mentioned by 42%; the imaginary divisibility of the form into elementary components was given by 24%; familiarity was mentioned by one *S*.

(7) A number of different bases were given for symmetry. Two-thirds defined it as being the opposition of similar parts across an axis which was usually vertical but for some could be horizontal or oblique. Some arranged the forms on the basis of a rotational symmetry, and several infrequent bases were given.

(8) A correlation of only 0.21 was found between the 'esthetic measure' of the forms and the average preference of the group. Birkhoff's formula gives high esthetic value to simple forms whereas it was the complex ones that were generally preferred by our group.

THE ROLE OF THE AUDITORY AREA OF THE CORTEX

By EDWARD GIRDEN, Brooklyn College

The ability to localize laterally produced sounds is a function of some differential produced at the peripheral acoustic apparatus. These left-right orientations normally require the use of both ears, and depend upon a difference in the properties of the stimulus impinging upon the cochleas: intensity, phase, or time differences. It has been shown that the cortical participant in this behavior is the auditory cortex of the left, or the right cerebral hemisphere.[1] This 'safety' factor is such that in the dog the L-R discrimination can be continued uninterrupted or initially acquired after hemidecortication.

Similar findings had been previously reported in the white rat by Pennington.[2] From his data, he concluded that the auditory cortex was equipotential, since errors in the L-R discrimination increased in proportion to the amount of cortex extirpated. For stimuli Pennington had used buzzers, which introduced an experimental artifact invalidating his interpretation. That is, the percentage of errors would have also increased in a rat with *intact* cortex if the complexity of the sound source had been reduced from gross noise to a simple sine wave such as a pure tone.[3] This is supported by the fact that the L-R discrimination has not been procured in the *normal* white rat for pure tones.

If the auditory cortex is 'equipotential' then, whatever interpretation holds for the action of the peripheral acoustic apparatus, a frequency theory of hearing is the only satisfactory description of the function of this mechanism. That is, pitch would be determined by the volleys of impulses set up anywhere within the critical limits of the cortical auditory area. On the other hand, if specific parts of the auditory area mediate particular pitches, then some form of a place theory obtains at the cortical level, as it has been ascribed to the more peripheral acoustic mechanisms. These

* Accepted for publication September 30, 1939. From the Psychological Laboratory, Brooklyn College. Special aid from the Elizabeth Thompson Science Fund, and the Penrose Fund of the American Philosophical Society, which made possible this research, is gratefully acknowledged. This study was reported at the Atlantic City meetings of the Eastern Psychological Association, April 6, 1940.

[1] E. Girden, Cerebral determinants of auditory localization, this JOURNAL, 52, 1939, 1-15.

[2] L. A. Pennington, The function of the brain in auditory localization: IV. Method of training and control experiments, *J. Comp. Psychol.*, 25, 1938, 195-211.

[3] Girden, *op. cit.*, 14.

implications of Pennington's study have until now been overlooked both by him and other workers in the field.

A critical test of this question was initiated by determining the effect of bilateral extirpation of the auditory area upon (1) the L-R habit for pure tones as well as bells, and (2) auditory acuity for the octave frequencies from 256 ~ to 8192 ~. If hearing is impaired more on some frequencies than on others, or the L-R habit is disrupted for some pitches and not others, evidence would be had for a place theory. The opposite results would be in support of some form of a frequency hypothesis.

A description of the method by which the L-R habit was established is already available.[4] Briefly, the dog was conditioned to flex right forepaw to a pure tone or a bell sounded from its right side, but to inhibit this response whenever the stimulus was presented from its left side. The intensity of the bell was fixed, and was much louder than any pure tones capable of being produced by the audiometer. The pure tones were just loud enough to permit the animal to make immediate reactions upon their presentation. Auditory acuity at the respective octave pitches was procured by a standardized procedure.[5] The cortical operations were performed aseptically under Nembutal anesthesia.

RESULTS

Protocols of the experiments on Dogs 7, 8, 9, and 10 are as follows.

Dog 7. Restricted portions of the sylvian gyri, areas 78 and 78b,[6] were extirpated in a single operation on 7/13. The first post-operative retention-test was made 48 hr. later (Table I). The L-R habit for the bells was present, but could not be evoked for the pure tones with the loudest sounds available. Unconditioned L-R head orientations were evoked by the tonal stimuli. On the second test (the fifth day after the operation), the L-R habit reappeared on all test frequencies. The intensities, however, had to be increased 10 to 15 db. over the pre-operative magnitudes. A similar process of post-operative inhibition and spontaneous recovery within five days has been reported by Marquis and Hilgard in the study of the conditioned response (CR) of the eyelid to light after the removal of the occipital lobes.[7]

A reëntry on the right side was made on 7/20, and a larger section of the sylvian gyri and some adjoining posterior ectosylvian cortex (area 68) were extirpated. On the first post-operative test the results were similar to those found after the first

[4] Girden, Conditioning and problem solving behavior, this JOURNAL, 51, 1938, 677-687.

[5] E. A. Culler, G. Finch, E. Girden, and W. J. Brodgen, Measurements of acuity by the conditioned-response technique, *J. Gen. Psychol.*, 12, 1935, 223-227.

[6] Cf. J. W. Papez (*Comparative Neurology*, 1929, 1-518) for the procedure used here in identifying the cortical areas.

[7] D. G. Marquis and E. R. Hilgard, Conditioned lid responses in dogs after removal of the visual cortex, *J. Comp. Psychol.*, 22, 1936, 157-178.

operation. Localization for the pure tones was suppressed whereas normal function for the bell was present. Again spontaneous recovery for all frequencies occurred on the second test (7/25), five days after this operation. Likewise the intensity of the pure tones had to be increased over the normal values; this time, 10 to 30 db.

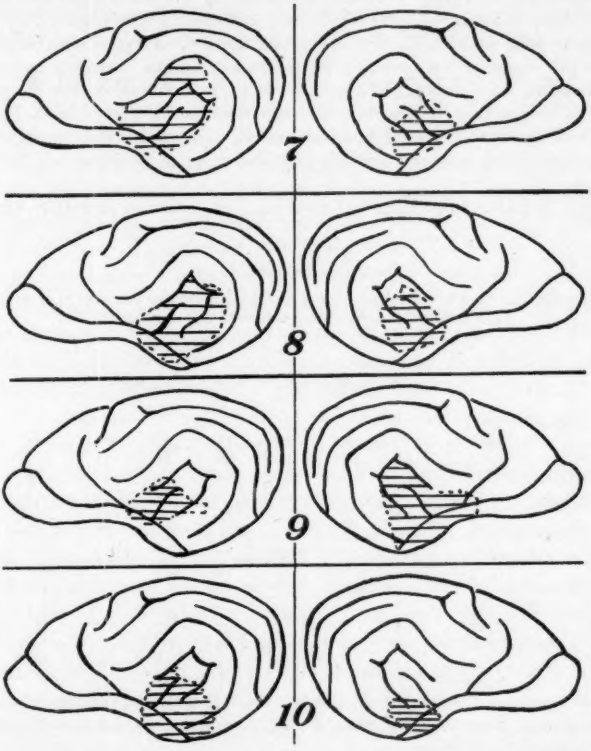

FIG. 1. THE CORTICAL EXTIRPATIONS FROM DOGS 7, 8, 9, AND 10
The extirpations are indicated by the lined areas. Single bilateral operations were performed on Dogs 8 and 9. Two reëntries were made on Dog 7 and one on Dog 10. The L-R discrimination was temporarily depressed in Dogs 7 and 10 and permanently abolished in Dogs 8 and 9.

On 8/3, a third and final operation was performed, consisting of a reëntry on the left side. The remainder of the sylvian gyri and some of the anterior ectosylvian gyrus (area 38b), anterior section of the middle ectosylvian gyrus (area 68b) were extirpated. The total amount of cortex removed in all three operations is shown in Fig. 1. Note that the dorsal cortex of the sylvian gyri on the right side had not

been removed. The first test after the third operation was completely negative for all stimuli. Again, unconditioned head orientations were noted. The second test was made on the tenth post-operative day, and the L-R habit had returned completely for all stimuli. Again the intensity of the pure tones had to be maintained at a louder level: 15 to 25 db. These results are summarized in Table I. It should be noticed that the increase in the intensity of the pure tones is not the same for all pitches. The longer delay between the first and second tests after the third entry was partly due to the effects of the operation. Motor incoördinations, including that of the right forepaw, were present for a number of days. The phenomenon of 'psychic blindness' was also noted. That is, pupillary reaction to light could be evoked, but the animal would walk blindly into objects, food pans, walls of the cage, and the like. Similar results in Rhesus monkeys have been reported by Klüver and Bucy.[8]

Dog 8. A bilateral operation was made on 7/27, as shown in Fig. 1. Ten days

TABLE I

RESULTS OF DOGS 7 AND 10

N indicates normal function; the figures show the decibel increase necessary to evoke the L-R habit; the dashes mean complete loss of function.

		Stimuli							
		256∼	512∼	1024∼	2048∼	4096∼	8192∼	9747∼	Bell
Dog 7	(Entry: 7/13)								
	7/15	—	—	—	—	—	—	—	N
	7/18	15	10	15	N	15	15	N	N
	(Entry: 7/20)								
	7/23	—	—	—	—	—	—	—	N
	7/25	20	30	10	10	20	15	N	N
	(Entry: 8/ 3)								
	8/ 5	—	—	—	—	—	—	—	—
	8/13	15	15	20	20	15	25	N	N
Dog 10	(Entry: 7/13)								
	7/18	5	10	5	15	15	5	5	N
	(Entry. 7/20)								
	7/23	15	10	30	15	15	10	N	N

after the operation the first test showed that the L-R habit was completely abolished for all stimuli, pure tones and buzzers alike. Unconditioned L-R head orientations to the sounds were procured.

Dog 9. The bilateral extirpation of auditory cortex was made on 7/28, as indicated in Fig. 1. A post-operative test on 8/4 was negative for all stimuli, bells and pure tones. Again, the presence of auditory function was indicated by head orientations, but the L-R habit, as in the last animal, was completely abolished.

Dog 10. Some of the posterior ectosylvian (area 68) and the ventral portions of the posterior sylvian gyrus (area 78) on both side were extirpated 7/13. Five

[8] H. Klüver and P. C. Bucy, "Psychic blindness" and other symptoms following bilateral temporal lobectomy in Rhesus monkeys, *Amer. J. Physiol.*, 119, 1937, 352-353.

days after the operation the L-R habit was unimpaired, although the intensity of the pure tones had to be increased 5 to 15 db. No response was procured on 128 ~.

A second entry on the left side was made 7/20. The lower portions of the sylvian gyri as well as some adjoining posterior ectosylvian and suprasylvian (area 70) cortex were extirpated. The final situation is indicated in Fig. 1. The first test after this entry was made on the third day, and the L-R habit was unimpaired. The intensity of the pure tones, as in the case of Dog 7, had to be increased (cf. Table I).

Of these four animals, the L-R habit functioned normally in Dogs 7 and 10, whereas it was completely abolished in Dogs 8 and 9. It was impossible to disrupt the discrimination for specific pitches in either Dogs 7 or 10. It is true that, in these two animals, the intensity of the pure tones had to be temporarily increased. More important, these changes were not equal for all pitches. The significance of this will become clear after the data of the next two animals are discussed below.

Failure to disrupt the L-R discrimination for particular pitches suggested the following possibilities. (1) A place theory is inapplicable to cortical auditory functions; hence no 'scotoma' could be produced, whereby the L-R habit would be lost for specific tones. (2) The cortical auditory area does function according to a place theory, but the experimental technique was unsatisfactory. That is, since the projection system from the medial geniculate bodies to the cortex is unknown in the dog, there is no apriori evidence to indicate where one might expect the radiations to terminate. Under these circumstances there might be some impairment in acuity but not the complete loss of hearing, if only a section of the area mediating a particular pitch is excised. There were other possible explanations, but in any case it seemed hopeless to continue along the present lines. Clearly, tests of auditory acuity throughout the pitch-range were required. Accordingly, in the next two animals, the thresholds for the octave frequencies from 256 ~ to 8192~ were approximated after the L-R habit had been established.

Protocols of the experiments with Dogs 13 and 14 are given here.

Dog 13. The bilateral ablation of the posterior sylvian gyrus was performed on 3/16, as represented in Fig. 2. Testing was initiated 48 hr. later. As can be seen in Table II, hearing for 256 ~ was completely depressed for the first two tests. The other pitches were present from the beginning, but with severe losses, varying in magnitude from pitch to pitch. As the tests continued these losses were gradually eliminated, and by the final test (eighth post-operative day), the acuity had returned practically to normal. There were some residual losses on 512 ~ and 1024 ~ which, by statistical tests, were greater than could be expected by chance.

Dog 14. The cortex removed bilaterally on 3/25 is indicated in Fig. 2. All hearing, including the L-R habit, was completely depressed. This continued for four

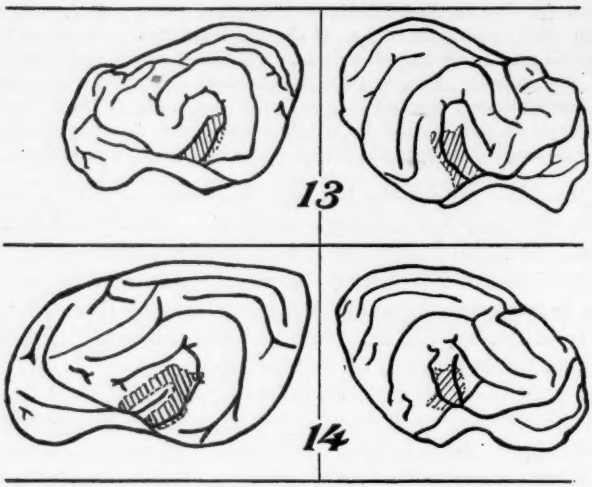

FIG. 2. THE CORTICAL EXTIRPATIONS FROM DOGS 13 AND 14

The extirpations are indicated by the lined areas. They were made bilaterally in a single operation in both animals. Large initial losses in hearing occurred which were followed by spontaneous recovery. Residual losses of 6 to 8 db. occurred in Dog 13 with 256 ~ and 1024 ~.

TABLE II

RESULTS OF DOG 13

The preoperative limen was set at 0. This does not mean that the thresholds at the different pitches are the same, but merely that all changes are measured from the normal level. The loss in hearing is, therefore, shown directly by its positive distance from zero. A minus value indicates a gain in hearing, *i.e.* it is better than the mean of the preoperative tests. The critical ratios **indicate** that the losses must be considered as real.

	Stimuli					
	256 ~	512 ~	1024 ~	2048 ~	4096 ~	8192 ~
Limen	0.0	0.0	0.0	0.0	0.0	0.0
σ_M	1.86	1.67	1.32	1.01	1.67	2.42
n	7	6	7	7	7	7
Mar. 18		38.7	66.9	60.9	73.1	72.7
Mar. 19		51.7	36.4	20.4	22.1	20.7
Mar. 21	−6.1	4.2	11.4	20.4	34.6	13.2
Mar. 22	3.9	9.2	11.4	12.9	19.6	8.2
Mar. 23	6.4	11.7	6.4	5.4	12.1	10.7
Mar. 24	1.4	9.2	6.4	0.4	−0.4	0.7
Loss	1.4	8.6	6.4			
σ_M	2.72	1.58	0.0			
n	4	4	2			
Critical ratio	0.47	**3.73**	**4.84**			

successive tests (3/27-30 inclusive). Head orientations were present on the very first test. On the second test the "attentive body-set," which well-trained animals possess, reappeared. On the fourth test "some incipient CR behavior" was observed and that "spontaneous recovery of the function may occur."

On 4/2 (eighth post-operative day) the fifth test was made, and the L-R habit now appeared in full strength for all frequencies. There still was considerable attenuation in hearing on 2048 ~ anl 4096 ~. Unlike the previous animal, Dog 13, normal hearing returns to Dog 14 at all frequencies (cf. Table III). When all five post-operative tests are averaged, a real loss is statistically indicated on only one pitch, *i.e.* to 4096 ~. This, however, does not take into account the progressive recovery of function. By the last test (April 11), the acuity returns to the normal level on all pitches.

The results procured with these two animals support the results obtained from Dogs 7 and 8 as well as the previously reported findings.[9] Unless

TABLE III

RESULTS OF DOG 14

	Stimuli					
	256 ~	512 ~	1024 ~	2048 ~	4096 ~	8192 ~
Limen*	0.0	0.0	0.0	0.0	0.0	0.0
σ_M	0.90	1.50	1.50	1.88	1.38	1.15
n	8	9	9	9	9	9
Apr. 2	2.2	−8.3	−11.9	22.2	16.7	2.8
Apr. 3			8.1	12.2	9.2	2.8
Apr. 4	−5.3	−3.3	0.6	4.7	9.2	−2.2
Apr. 6	−0.3	4.2	5.6	4.7	4.2	2.8
Apr. 11	2.2	−0.8	−6.9	−12.8	−0.8	0.3
Loss	−0.3	−2.05	− 0.9	6.2	7.7	1.3
σ_M	1.77	2.61	2.98	3.21	2.31	0.79
n	4	4	5	5	5	5
Critical ratio	0.18	0.68	0.27	1.77	2.86	.93

* For an explanation see Table II.

the critical auditory areas are removed completely from both hemispheres, permanent disruption of the L-R habit does not occur at any pitch.

More significant is the marked temporary impairment of acuity that occurs in both of the last animals. As shown in Fig. 2, the cortical areas extirpated were not identical in those two cases. Nor are the curves of attenuation in hearing the same for both specimens (cf. Tables II and III). In Dog 13, the severest losses first occur on the highest pitches with somewhat decreasing magnitude to the lower end of the pitch-range (excepting 256 ~, which was completely abolished for the first two tests). In Dog 14 the greatest losses occur on 2048 ~ and 4096 ~, with decreasing impair-

[9] Cf. Girden, *op. cit.*, this JOURNAL, 52, 1939, 1-15.

ment at both ends of the pitch range. These results are supported by those obtained from Dogs 7 and 10. The fact that the intensity of the pure tones had to be increased post-operatively in these two animals indicated an attenuation in hearing. Likewise the amount of impairment was not the same for all pitches (cf. Table I).

These initial severe hearing-losses, however, are followed by a spontaneous recovery of function which cannot be attributed to retraining. Little shock-reënforcement was used, and then only for the purpose of motivation. The sudden reappearance of the L-R habit for all pure tones also minimizes this possibility. The establishment of this discrimination, particularly for pure tones, requires periods of consistent shock-reënforcement training which usually last for many weeks. The longer period of depression in Dog 14 is at present inexplicable. It is unfortunate no tests were made in Dog 14 from the fifth to eighth post-operative days. Undoubtedly similar large differentia in acuity would have been noted as in Dog 13, because there is, on the first positive test (the fifth test), a difference of 34 db. between 1024 ~ and 2048 ~.

Discussion

Without presenting a minute description of the neuroanatomy, many details of which are still unknown, the following facts are relevant. The acoustic nerve, after its exit from the cochlea, divides into crossed and uncrossed components, such that each medial geniculate body receives impulses from both cochleas. That is, the left acoustic nerve contributes to both the left and right medial geniculate bodies. The fibers of the right acoustic nerve distribute in a similar manner. An important consideration for the understanding of the physiological basis of auditory localization is the work of Kemp and Robinson. They have shown that impulses originating in both ears (cochleas) pass up both the crossed and uncrossed auditory tracts at the same time. Another way of stating this fact is as follows. In the left lateral lemniscus (containing uncrossed tracts from the left ear and crossed tracts from the right ear), impulses from both cochleas go towards the higher nerve centers simultaneously.[10] No crossed pathways occur at the thalamus; all radiations from the left geniculate body project to the auditory cortex of the left hemisphere, and the fibers from the right geniculate body terminate on the cortex of the right hemisphere.[11]

[10] E. H. Kemp and E. H. Robinson, Electrical responses of the brain stem to bilateral auditory stimulation, *Amer. J. Physiol.*, 120, 1937, 316-322.

[11] F. Bremer and R. S. Dow, The cerebral acoustic area of the cat, *J. Neurophysiol.*, 2, 1939, 308-319.

Since each geniculate body receives impulses from both cochleas, each cortical area thus receives impulses from both ears. The corpus callosum is presumably the only means of direct communication between the two cortical auditory areas.

There is considerable evidence to show that, following this anatomic organization, there is a comparable functional arrangement such that hearing is bilaterally represented.[12] This organization is more than the simple participation of both cerebral cortices. Since the L-R habit is maintained in the hemidecorticated animal, there must be complete representation of both cochleas on each cortex.[13] There is every indication that the cortical acoustic mechanism involved in the L-R auditory localization habit is not indiscriminately spread over the cortex as a whole, as maintained by some authorities.[14] To the contrary, it is probable that the primary auditory functions are mediated by a proscribed cortical area.

The auditory cortex seems to play a passive rôle in L-R behavior, the real determinant having a peripheral locus. That is, the differentiating factor is some constituent component of the stimulus impinging upon the ears: phase, intensity, and time differences at the two ears. How are they represented physiologically? Phase and time differences can be considered identical in action.[15] This would mean that physically the sound reaches one ear before the other is stimulated, and as a consequence, physiologically, impulses would be initiated in one acoustic nerve before a comparable volley would be aroused in the opposite tract.

The third important factor, intensity, can likewise be shown to be a case of physiological time differences. Suppose the sound reaches both ears in exactly the same phase and at the same moment, differing only in being more intense at one ear (say, the right) than at the other ear (the left). This will arouse impulses (a) more quickly, *i.e.* they will have a shorter latency, and (b) a greater number of them will be in the right acoustic nerve than in heterolateral (left) tract. The change in latency appears to be effective only up to intensities of 30 db. above the threshhold.[16] If, in a particular case, only the second factor is operative, this will again be translated in terms of physiological time-differences at the second neuron. That is, the greater burst of impulses in the first neuron from the cochlea of the right ear will activate the second neuron before a

[12] Cf. W. J. Brogden, E. Girden, F. A. Mettler, and E. Culler, Acoustic value of the several components of the auditory system in cats, *Amer. J. Physiol.*, 116, 1936, 252-261.
[13] Cf. Girden, *op. cit.*, this JOURNAL, 52, 1939 1-15.
[14] I. P. Pavlov, *Conditioned Reflexes*, 1927, 1-430.
[15] Cf. R. S. Woodworth, *Experimental Psychology*, 1938, 518-538.
[16] S. S. Stevens and H. Davis, *Hearing, its Psychology and Physiology*, 1938, 385.

comparable volley is initiated at the corresponding heterolateral level.

In other words, auditory localization is maintained physiologically in the respective auditory tracts by the time differences which have originated at the two cochleas. This relationship will be maintained through the second, third, and fourth neurons to both cerebral cortices, one pathway maintaining a lead over the other to the motor outlet which will produce the L-R orientation.

It has been mistakenly implied that there must be a one to one relationship between the frequency of the stimulus and the rate of discharge in the nerve. Thus Woodworth states, "Unless the impulses coming into the brain center from the two auditory nerves preserved the frequency of the stimulus, they could not preserve the binaural time-difference of the waves in a continuous tone, and this essential cue of direction would be lost."[17] Since the first report of the Wever and Bray phenomena in 1930, there has been much needless discussion concerning this relationship. Their original results indicated a concomitance between the acoustic frequencies and the electric phenomena, up to 4100 ~. This caused considerable concern, since there was an apparent contradiction between these findings and the all-or-none law and the known facts of refractory period.

More recent studies have shown that this effect holds only for the cochlear microphonics. As far as the cerebral acoustic pathways are concerned, "no synchronized impulses at frequencies above 100 per second have been detected from the auditory cortex or auditory radiations."[18] There need be no greater expectancy for a one to one relationship between the frequencies of the acoustic stimuli and the resultant auditory nerve impulses any more than one would expect the frequencies of the visual stimuli to be directly expressed into a numerically equal volley of nerve impulses in the optic nerve. All that would be required is that the *relative differences* be represented, whether the volley of nerve impulses be 5, 10, 50, or 100 per sec.

Since the discrimination persists following transection of the corpus callosum, or hemidecortication, there must be a common motor pathway from both auditory areas. The question that remains unanswered is the origin of and the pathways from the auditory cortex to the motor pathways.

It has been suggested by Culler that the auditory system, like the visual and somesthetic modalities, consists of a projection system such that the anatomic arrangement of the peripheral mechanism is maintained throughout the central nervous system up to and including the cortical terminus.

[17] Woodworth, *op. cit.*, 537.
[18] Stevens and Davis, *op. cit.*, 402.

Evidence for this view and support for the consequent place theory of hearing has been offered for the cochlear mechanism and the medial geniculate bodies in the thalamus.[19] Detailed analyses of the relevant data and the theoretical implication thereof are available elsewhere.[20]

The present data offer support for this hypothesis. If the auditory cortex were completely equipotential and did not function according to some such principle, it would be difficult to explain the unequal *losses* on the *different* pitches following restricted lesions in the auditory area. Evidently, different parts of the acoustic cortex, in the first instance, function in the perception of different pitches. The differences produced between the two animals (cf. Tables II and III) are perhaps not as marked as those procured on the thalamic level by Ades.[21] Since the present study was a preliminary analysis, the lesions produced here are not as restricted, nor as different in the two animals (cf. Fig. 2). The present results, therefore, support the hypothesis that the primary principles of action of the cortical acoustic system is some form of a place theory, whereby different parts of the auditory area are activated by different pitches.

Certain differences between the present data and the findings reported at the cochlea and the thalamus suggest, however, that the cortical mechanism is more complicated than implied by a simple place theory. In the present experiment there were relatively no permanent impairments in acuity. In any case, they are not comparable to these produced at the thalamus. The initial sharp losses are eliminated within a week or so through a process of spontaneous recovery of function. This occurs, of course, only if some part of the primary auditory area remains intact.[22] Obviously

[19] Cf. H. W. Ades, F. A. Mettler, and E. A. Culler, Effect of lesions in the medial geniculate bodies upon hearing in the cat, *Amer. J. Physiol.*, 125, 1939, 15-23; S. S. Stevens, H. Davis, and M. H. Lurie, The localization of pitch perception on the basilar membrane, *J. Gen. Psychol.*, 13, 1935, 297-315.

[20] Cf. Stevens and Davis, *op. cit.*, 356-436; Woodworth, *op. cit.*, 501-538; E. G. Weaver, The electrical responses of the ear, *Psychol. Bull.*, 36, 1939, 143-187.

[21] Ades, Mettler, and Culler, *op. cit.*, 15-23.

[22] It is also true that even if all of the auditory cortex is removed bilaterally, the animal does not become permanently deaf. Within two weeks, auditory function does return, but now at a *subcortical* (thalamic) level. As evidenced in the chronic decorticate dog, not only is auditory function present, but an auditory CR and even a tonal discrimination may be established. (Cf. E. Girden, F. A. Mettler, G. Finch, and E. Culler, Conditioned responses in a decorticate dog to acoustic, thermal, and tactile stimulation, *J. Comp. Psychol.*, 21, 1936, 367-385.)

Complete bilateral extirpation of the auditory cortex, however, does *disrupt* the CR as well as the L-R localization habit upon which it is based. While spontaneous recovery of auditory function occurs, after the usual period of post-operative rest, as shown by head-orientations to sound stimuli, the CR is permanently abolished. Retraining, of course, will reëstablish a *new* CR. But this is not the spontaneous recovery that does occur, in the present study, without retraining.

Similar results were present in the totally decorticate specimen. Conditioned

a place theory under these circumstances is no longer tenable. This suggests the following hypothesis. At the cortex, unlike the more peripherally located components of the acoustic system, there is in operation another mechanism that for the present is best described as equipotentiality. If the primary projection system is disrupted by localized lesions, normal functions would now be maintained in this emergency by the relative frequency or volleys of nerve impulses in the remaining functional auditory cortex.[23]

This would explain why it was not possible to eliminate the L-R habit for specific frequencies or produce severe permanent losses in hearing. When the primary projection system (place theory) is disturbed by localized lesions, pitch is then maintained by the respective volleys of impulses (frequency theory). Presumably, normal hearing should be maintained so long as some part of the primary acoustic area remains functionally intact.

This concept is not entirely foreign nor contradictory to other known facts of cortical functions.[24] A somewhat analogous situation occurs in the clinical condition of hemianopsia. Total blindness of corresponding halves of the visual field of both eyes should follow total destruction of the primary visual cortex of one hemisphere. Yet objects can be recognized within an area where stimulation, during perimetrical examination, is ineffective. According to these studies of Goldstein, "precise exploration shows that the patients are not limited to half of a field of vision, but that their field of vision is arranged around a center like normal, and that, likewise, the region of their clearest vision lies approximately at this center. . . . Apparently, that point in the outer world seems clearest which is reflected not on the border of the intact retina, where the old macula now lies, but on an area *within* the intact retina."[25] In other words, following disruption of the visual projection system dynamic reorganization takes

responses established after the third operation were permanently abolished subsequent to the fourth and final operation, which was the last stage in complete decortication. After this last entry, the CR never spontaneously reappeared and only occurred afterwards following subsequent training. (Girden, Mettler, Finch, and Culler, *op. cit.*, 373 f.) In the present study, therefore, the fact that there was spontaneous recovery of auditory function is in itself not indicative of cortical activity. But, simultaneously, and as an integrated part, came the return of the CR and the L-R habit *without retraining*. This does indicate that the pattern, therefore, was still cortical and hence can be attributed to the functioning of the remaining auditory cortex, and not indicative of thalamic activity.

[23] Wever, *op. cit.*, 175 ff.

[24] Cf. K. S. Lashley (The mechanism of vision. XVI. The functioning of small remnants of the visual cortex, *J. Comp. Neur.*, 70, 1939, 45-67) who has recently shown that the discrimination of visual figures in the rat does not depend on a particular part of the striate areas. So long as any section of the geniculostriate system remains intact, even a few cells, appropriate reactions are made.

[25] K. Goldstein. *The Organism*, 1939, 47 f.

place in the remaining optic cortex so that normal functions are maintained. Likewise, in the present case, in the study of a different sense modality hearing, similar dynamic readjustments occur.

SUMMARY

Evidence is offered to show that there is a specific cortical auditory area, which when extirpated will abolish the L-R habit as well as the CRs upon which it is based. Restricted lesions within the limits of this area will only temporarily depress this function, spontaneous recovery occurring within a period of about five days. It is not possible to disrupt the L-R discrimination for specific pitches.

During the period of depression following restricted lesions, auditory acuity is also impaired. Complete recovery of function occurs after a longer period of time; approximately seven to eight days. The severe temporary impairments in acuity differ in the two animals from pitch to pitch.

To explain these facts, it is suggested that a primary projection system of the peripheral acoustic mechanism exists at the cortex, whereby particular pitches are mediated by different areas (place theory). But in emergencies, such as produced by ablation of restricted portions of the auditory area, normal function is maintained by a principle of equipotentiality. That is, the intact auditory cortex is still capable of mediating normal hearing according to the number of nerve impulses produced by the respective tones (frequency of volley theory).

CHANGES IN GENERAL BEHAVIOR DURING FLEXION CONDITIONING AND THEIR IMPORTANCE FOR THE LEARNING PROCESS

By IRVIN S. WOLF and W. N. KELLOGG, Indiana University

The conditioned response method has often been thought of as the most refined and accurate technique available for the study of learning. It limits its observations to the minute analysis of one simple act. If precise recording devices are employed, it permits a quantitative study of the movements or responses produced to a degree of perfection as yet unapproached by any other method. Yet its greatest virtue is also its most serious deficiency. For by studying intensively the modifications which take place in one organ or part of the body, it has tended to overlook the activity of other parts not directly concerned. It has turned a spotlight upon a portion of the learning process, but has left the remainder largely unexplored. The alterations through which the whole living organism passes must be clearly observed and studied for a thorough understanding of the genesis of learning.

Zener, in a recent article, has attempted to make up for this deficiency by supplying descriptions of the changes in the general behavior of dogs during salivary conditioning.[1] He writes as follows, "The relative inadequacy of available experimental facts to differentiate decisively between the alternative theories of conditioning is due largely to the circumstance that the traditional method reflects the substitute stimulus conception held by the original Russian investigators, and is thus not well designed to reveal facts conflicting with it. Thus with the salivary response, although concomitant occurrence of overt behavior has been reported, the fact that it has not been described in any detail reflects the opinion that it is incidental and irrelevant to the response selected for study."[2]

With regard to the conditioned withdrawal reaction, much the same situation exists although there has been, perhaps, more notice of the "incidental" behavior which accompanies the training than in the case of the salivary reaction. As early as 1916, Watson wrote upon this subject, "The conditioned motor reflex is usually

* Accepted for publication January 30, 1940. This report, from the Indiana Conditioning Laboratory, was made possible by a series of grants-in-aid of research from the University, and by continual assistance from the National Youth Administration of the United States Government.

[1] K. Zener, The significance of behavior accompanying salivary secretion for theories of the conditioned response, this JOURNAL, 50, 1937, 384-403.

[2] *Ibid.*, 388.

sharp, quick, and widespread, the whole body as a rule being brought into the reaction at first. Gradually the reflex becomes more circumscribed."[3] Beritoff noted barking, howling, groaning, tossing, vocal, and other reactions of his dogs when in the apparatus,[4] while Bechterev has pointed out that "the association reflex is, to a certain extent, more or less general in character when it appears, and only gradually, hand in hand with the fixation of the association reflex, does it become more and more local, until, finally, it is expressed in one defensive movement of the organism stimulated."[5]

Among the later American students of this question, Liddell has observed that "in the early stages of training, the reactions to the shock and to the signal that precedes it are generalized and impulsive to a high degree. . . . With increasing training, a gradual process of concentration and individuation supervenes."[6] Supporting this general finding, Culler and his associates write "The localized, individuated response of a well-trained dog is as unlike his original excited, diffuse behavior as two acts can well be."[7]

In several different papers, Schlosberg, and Kappauf and Schlosberg have reported conditioned struggle behavior in rats with which they worked and have observed that this struggle decreased with training.[8] Both tail withdrawal and foreleg withdrawal were studied. Schlosberg noted further that one stock of rats struggled more than another and that the strugglers as a rule learned more quickly.[9] "Pre-training" or habituation served to eliminate some struggle and probably to retard the conditioning. In another paper, Kappauf and Schlosberg suggest, on the other hand, that the production of excitement is not conducive to the development of precise responses.[10]

Kellogg and Walker, studying the bilateral transfer of the conditioned flexion-response in dogs, were not so confident that the conditioned withdrawal was specifically an outgrowth of the earlier struggle-reaction.[11] Some dogs they noticed struggled a great deal, while others in the same situation struggled very little. This problem has been followed up by Kellogg and Wolf who have presented data which

[3] J. B. Watson, The place of the conditioned-reflex in psychology, *Psychol. Rev.*, 23, 1916, 97.

[4] J. S. Beritoff, On the fundamental nervous processes in the cortex of the cerebral hemispheres. I. The principal stages of the development of the individual reflex: Its generalization and differentiation, *Brain*, 47, 1924, 109-148; On the fundamental processes in the cortex of the cerebral hemispheres. II. On the principal cortical elements in the arcs of the individual reflexes, *ibid.*, 47, 1924, 358-376.

[5] V. M. Bechterev, *General Principles of Human Reflexology*, 1933, 337.

[6] H. S. Liddell, The conditioned reflex, Chap. IX in F. A. Moss' *Comparative Psychology*, 1934, 272.

[7] E. Culler, G. Finch, E. Girden, and W. Brogden, Measurements of acuity by the conditioned-response technique, *J. Gen. Psychol.*, 12, 1935, 224.

[8] H. Schlosberg, Conditioned responses in the white rat, *J. Genet. Psychol.*, 45, 1934, 303-335; Conditioned responses in the white rat: II. Conditioned responses based upon shock to the foreleg, *ibid.*, 49, 1936, 107-138; The relationship between success and the laws of conditioning, *Psychol. Rev.*, 44, 1937, 379-394; W. E. Kappauf and H. Schlosberg, Conditioned responses in the white rat: III. Conditioning as a function of the length of the period of delay, *J. Genet. Psychol.*, 50, 1937, 27-45.

[9] Schlosberg, *op. cit.*, *J. Genet. Psychol.*, 45, 1934, 324.

[10] Kappauf and Schlosberg, *op. cit.*, *J. Genet. Psychol.*, 50, 1937, 43.

[11] W. N. Kellogg and E. L. Walker, "Ambiguous conditioning," a phenomenon of bilateral transfer, *J. Comp. Psychol.*, 26, 1938, 63-77.

show that struggling or generalized behavior may exist quite independently of the conditioned flexion-withdrawal.[12] Their findings on this point are summed up in the following statement, "Some animals, although they develop the conditioned flexion-response, never struggle at all. Others may struggle almost continuously, probably in response to the confining experimental situation, but develop the conditioned flexion-CR as an entirely independent reaction. They may never eliminate struggle even if given hundreds of conditioning trials."[13]

PROBLEM

Most of the data supplied by earlier investigators has been based on general observation during conditioned-response training. It is supported principally by descriptive protocols which have necessarily been qualitative in nature. This has been an unavoidable result of the fact that methods were not available for recording the reactions of any part of the organism except the particular segment selected for study.

The present experiment was designed specifically to overcome this difficulty. It makes use of a technique for recording simultaneously the reactions of all four limbs of the *S* in addition to his respiration. This procedure makes possible a more accurate and detailed picture of the general behavior which takes place than it has heretofore been possible to obtain.

Is the specific or adaptive CR actually demonstrated to be an outgrowth of the earlier generalized activity, when this generalized activity is in part recorded and hence subject to more exact analysis? Does some other principle furnish a better description of the changes which take place? Is any single description applicable to all cases? Answers to these and similar questions should now be subject to quantitative study. The technique seeks thus to obviate the early difficulty of too concentrated an emphasis upon a portion of the organism, which was formerly inherent in the conditioned reflex method. It represents an attempt to quantify the random behavior of the *S* and to reduce it to some measurable value.

EXPERIMENTAL CONDITIONS

Subjects. Six unselected mongrel dogs supplied by an animal dealer were used as *S*s. These were all young but mature specimens, 20-30 lbs. in weight. Two were females and four were males. One of the females died suddenly when the experiment was half completed. Her data have been included in the grouped results, however, since they were found not to alter the general trends in the results of the other dogs, but served somewhat to smooth the graphs presented.

---

[12] W. N. Kellogg and I. S. Wolf, "Hypotheses" and "random activity" during the conditioning of dogs, *J. Exper. Psychol.*, 1940 (in press).

[13] *Ibid.*

Procedure. The apparatus and buzz-shock conditioning technique were similar to those employed in previous studies and have been described in greater detail elsewhere.[14] During all the training *S* was confined in a conditioning stock in a small soundproofed room, where he could be observed through one-way-vision windows. The head of the animal was fastened in a neck-piece adjustable for both size and height, and a leather belt was passed loosely under the abdomen. Each of the four feet was strapped to a balanced lever which permitted a maximum vertical movement of approximately 4 in. A Sumner pneumograph was placed around the chest just back of the shoulders. Kymographic records of the movements of all four feet, the respiration, together with the stimuli and time, were obtained in the experimenter's room outside the soundproofed room.

The conditioned stimulus was a pure tone of 1000 ~ with an intensity of 50-55 db. above the human threshold at the position of the dog's ears. It was sounded for 2 sec. on each trial. The unconditioned stimulus was a make-break D.C. shock of 0.2-sec. duration, which overlapped the buzz for the last 0.2 sec. of its 2-sec. period. The shock was delivered through stainless-steel electrodes taped to the clipped surfaces of the animal's right-hind foot just above the toes. It was adjusted for every *S* before each daily session, so as to be just strong enough to produce the maximal unconditioned flexion response of 4 in. permitted by the apparatus. If *S* made a conditioned lift of about 2½ in. near the end of the buzz-period, this automatically operated a small mercury switch which eliminated the shock-stimulation for that trial.

A day's training consisted of 20 stimulations, spaced 15 to 120 sec. apart. Sixteen of these trials were pairings of buzz and shock, while one trial in every group of 5 was a buzz-alone stimulation. The order or position of the no-shock trials was randomly varied within each group of 5. A complete training series included 400 trials (20 days) of practice. Between the 200th and 201st conditioning trials of the present experiment, an interval of from 2 to 4 mo. was allowed to elapse.

Measurements from records. The data discussed in this report are based upon measurements taken from the kymographic records. They include the relative frequency of CRs within each daily group of 20 stimulations, the amplitude or height of lift, and the latency. The principal criteria of measurement which were adopted are given below. They apply to the responses of each of the four feet.

(1) All measurable changes in contour of the record-line describing the movement of the foot were considered as responses. To be classified as *conditioned flexion-responses* they had to be movements in an upward direction. Furthermore they had to occur either (a) within the 1.8-sec. buzz-period preceding the shock or (b) within the 2.0 sec. of buzz-alone during the no-shock trials.

(2) In some trials, activity (such as struggling) was in progress before the beginning of the buzz. If this activity did not change with the presentation of the stimulus, the trial was tabulated as doubtful or uncertain with reference to CRs. It could not be determined for trials of this nature, whether the recorded behavior was or was not influenced by the conditioned stimulus.

[14] Kellogg and Walker, An analysis of the bilateral transfer of conditioning in dogs, in terms of the frequency, amplitude, and latency of the responses, *J. Gen. Psychol.*, 18, 1938, 253-265; *op. cit.*, *J. Comp. Psychol.*, 26, 1938, 63-77; W. N. Kellogg, R. C. Davis, and V. B. Scott, Refinements in technique for the conditioning of motor reflexes in dogs, *J. Exper. Psychol.*, 24, 1939, 318-331.

(3) The amplitude of the conditioned lift was the maximal height or rise in the line describing the movement of the limb, during the buzz-period.

(4) The latency of the CR was the time between the beginning of the buzz and the break in the recording line for the foot indicating the beginning of a lift. It was measured to the nearest 0.1 sec.

RESULTS

Conditioned responses in different limbs. The most adaptive type of CR from the point of view of the experimenter was a simple lift of the RR member beginning just before the time for the shock and just high enough to escape the shock. Yet at various stages during their training one or another of the *Ss* gave "adaptive CRs"—that is, CRs without struggle or violent activity—in all four feet, in three feet, in two, or in only one of the feet, which might or might not be the shocked member.

Figs. 1, 2, 3, and 4 show instances of CRs involving from one to four feet even though the right-rear foot only was shocked. In all these figures, the top line, R, is the respiration, LF, RF, LR, and RR are the tracings made by the left-front, right-front, left-rear, and right-rear members, and B, S, and T are the buzz, shock, and time-lines. Fig. 1 shows 'adaptive' responses to the buzz in all four feet during each stimulation. In reactions of this sort the different limbs were either flexed alternately in rapid succession, or else *S* supported his weight by means of the collar of the stock and the abdominal strap. Fig. 2 is similar except that the left front foot is entirely quiet. In Fig. 3, *S* has lifted only the rear members during the buzz period, while Fig. 4 shows flexion—CR of the RR limb which was high enough to escape the shock-stimulus. Conditioned respiration is also evident, especially in the responses reproduced in Figs. 1, 3, and 4. It shows itself by a change in the conformation of the breathing curve during the period of the conditioned stimulus.

Frequency of CRs. Composite graphs of the CRs made by all four limbs separately are shown in Fig. 5. The heavy solid line gives the frequency of CRs for the right-rear foot, the heavy broken line is for the left-rear, the light solid line is for the right-front, while the light broken line shows the frequency for the left-front foot. The break in all the curves after trial 200 indicates the time interval of 2 to 4 mo. which was interpolated in the middle of the training. Since one animal died during the course of the experiment, the graphs for trials 1-200 include the data from six dogs, while those for trials 201-400 are based on the results of five. Each point on the frequency curve represents the percentage of CRs given on each day of training. Doubtful or uncertain trials were eliminated from the figures by subtracting their number from the maximum possible in

computing the final percentage. When results for several feet or for the same foot on different animals were grouped (as in Fig. 5) this was

FIG. 1. FLEXION CRs IN ALL FOUR FEET

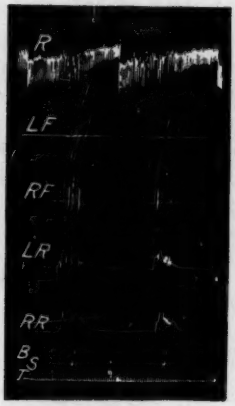

FIG. 2. FLEXION CRs IN THREE FEET

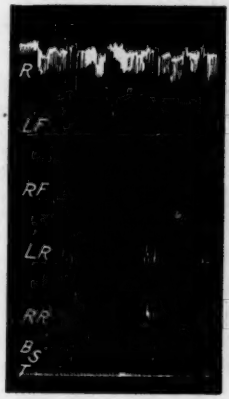

FIG. 3. FLEXION CRs IN TWO FEET

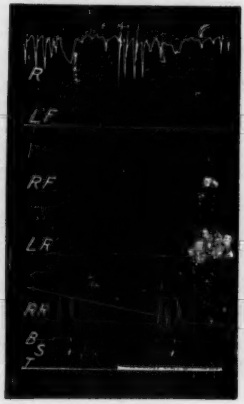

FIG. 4. A CR IN THE SHOCKED MEMBER ONLY

done by adding the total number of possible responses with doubtfuls omitted. Every point in the first half of the curves is therefore computed on the basis of 120 trials (less doubtfuls)—*i.e.* 20 daily trials for each

of six dogs—while those for the second half of the curves are based upon 100 stimulations (less doubtfuls).

The graph for the RR (shocked) foot is the typical conditioning curve which is obtained when frequency of flexion CRs is plotted. Although irregular, especially during the first 200 trials, the RR graph possesses something of the S-shaped contour which has been previously reported in conditioning experiments. The efficiency of learning increases as the curve rises from left to right.

This is not the case, however, in the curves for the three non-shocked feet. Since the lifts made by these members were superfluous or un-

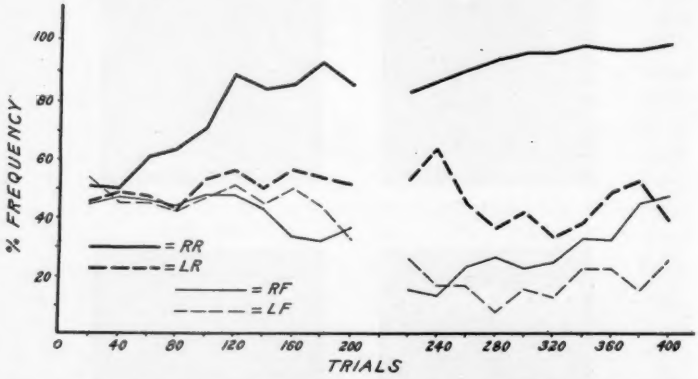

FIG. 5. COMPOSITE RELATIVE FREQUENCY GRAPHS OF THE FLEXION RESPONSES MADE BY ALL FOUR FEET DURING THE PROCESS OF DEVELOPING AN "ADAPTIVE" CR IN THE RR FOOT ONLY

Trials 1-200 were plotted from the data of six animals; trials 201-400 were plotted from the data of five. The break in the curves indicates an interpolated time interval of 2-4 months.

necessary, the efficiency of learning increases as the graphs drop or fall from left to right. The CRs of the three non-shocked members are in effect 'errors,' and may be considered as equivalent to the errors plotted in the orthodox trial-and-error experiment.

When the results of all the *Ss* are combined in this manner, it is clear that in the early trials the *Ss as a group* lifted all four feet with about equal frequency. As training progressed, the frequency of responsiveness of the shocked member increased at the same time that the frequency of responsiveness in the non-shocked members decreased. We have here different sorts of learning curves depicting the activity of different parts of

the body which took place simultaneously in the same experimental situation. The *S*s were clearly learning two things at the same time. The graph of the shocked (RR) member shows how they learned *what to do.* The graphs of the other three feet show how they learned *what not to do.*

A definite trend or progression in the frequency of responsiveness of the different feet is apparent especially during the latter part of the training. *Those members which were anatomically closer to the point of electrical stimulation responded with greater frequency than the more*

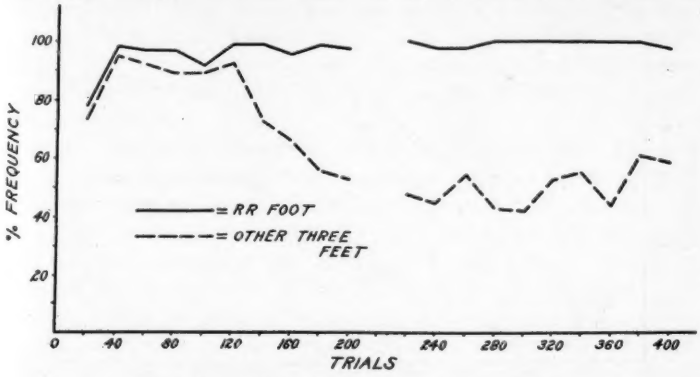

FIG. 6. FREQUENCY OF CRS FOR THE EXCITABLE *S*s

distant parts. Thus the order of frequency of responsiveness was RR, LR, RF, and LF.

The graphs show very little detrimental or forgetting-effect as a result of the interpolated time interval of 2-4 mo. After the time interval the curves seem to continue the trends established before it, with but minor variations. Such a result substantiates an earlier finding from this laboratory to the effect that the nature of the behavior after intervals up to 6 mo. is essentially a reproduction of that at the beginning of the intervals.[15]

The writers would like, at this point, to suggest a certain caution in the interpretation of the results shown in Figs. 1, 2, 3, 4, and 5. These results should *not* be taken to mean that in the case of all dogs, the first CRs to appear are present in all four feet, and that the superfluous feet are then successively eliminated. It is doubtful if such precision can be found in the case of any single *S*. The presence of CRs in the non-shocked members

[15] Kellogg and Wolf, The nature of the response retained after several varieties of conditioning in the same subjects, *J. Exper. Psychol.*, 24, 1939, 366-383.

should be thought of rather as a matter of individual variation and fluctuation from *S* to *S*. While there are certain general trends, which show up when the data are grouped, there are also marked individual differences. Dog 17, for example, raised its right-front foot a much greater number of times than either of the other non-shocked feet. This foot was often pumped up and down hard and fast during the presentation of the conditioned stimulus. The amplitudes for the responses of this foot, even when the movements become fewer in number, remained greater than the amplitudes for the RR or for any other limb. Comparable individual

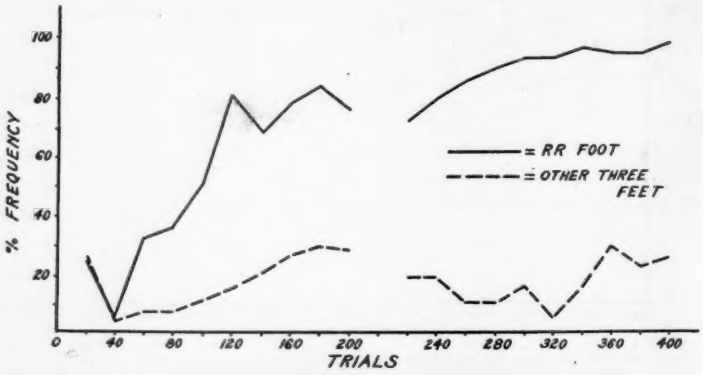

FIG. 7. FREQUENCY OF CRs FOR THE QUIET *S*s

peculiarities were also observed in the other *S*. These are to a considerable extent cancelled out in the group curves.

Excitable and quiet animals. An examination of the separate performances of the different animals shows that three of the *S*s had a much greater propensity to lift the non-shocked members than the other three. They may well be characterized as 'excitable' animals. These *S*s displayed a great deal of so-called random behavior during their conditioning which was not present in the other dogs. This included struggling, barking, howling, excessive tail movements, a large number of limb movements between trials, biting of the stock in response to one or the other of the stimuli, and defecation and micturition. Since the other three animals displayed but little of this sort of behavior, they may properly be characterized as 'quiet' animals.

If now the frequency scores of the three excitable animals are grouped together and the frequency scores of the three quiet animals are similarly

grouped, the results given in Figs. 6 and 7 are obtained. In each of these figures the solid line represents the frequency of the RR foot and the broken line is the composite frequency for the other three feet. Fig. 6 shows that the excitable animals began their training with a frequency of responsiveness in all four feet of over 70%, whereas the quiet animals (Fig. 7) began with a frequency of less than 20%. In the case of the excitable animals, the CRs in all four feet were near 100% for 120 trials. At this point a gradual reduction in the responsiveness of the non-shocked members began, while the shocked foot remained near the 100% level. The quiet animals, on the other hand, at no time lifted the three non-shocked feet with a composite frequency of greater than 30% and did not arrive at the 100% level with the shocked member until they had received almost 400 trials.

What the excitable animals had to learn was *what not to do*. Yet they never succeeded in reducing their non-shocked CR much below the 50% level. What the quiet animals had to learn was *what to do*. This they did in a gradual manner—the responses of the non-shocked feet remaining at a low level throughout their training. The quiet group of *Ss* has an S-shaped conditioning curve for the RR (or shocked) foot. There is no evidence of this phenomenon in the case of the excitable animals.

Such results preclude any general inference to the effect that the adaptive CR regularly develops from the earlier conditioned struggle of the animal. A statement of this sort may hold for some *Ss*, but it certainly does not apply to all.

Amplitude of CRs. Analogous curves prepared from the amplitude data gave strikingly similar, though not identical, results. This is not because one of these measures is a function of the other. The average amplitude of lift was obtained by totalling the number of inches lifted in each day's training, and dividing this total by the number of CRs made. Average amplitude as computed in this way is not affected by the frequency of CRs, but is the average height of the responses that do occur. This makes the two measures independent.

The excitable *Ss* made flexion CRs which were of greater extent on the average than those of the quiet *Ss*. The average height of lift in the non-shocked feet became gradually less for the excitable animals, although it never dropped much below 2 in., which was one-half the maximum distance permitted by the apparatus. CRs made by the quiet animals were of small amplitude at first although they gradually increased in amplitude—following an S-shaped contour—as training progressed. Curves derived from the amplitude data were, in fact, so similar to those already presented

for frequency of CRs that it is unnecessary to reproduce them here.

Latency of CRs. With regard to latency, however, the differences in the behavior of the two groups tended to disappear. The latency of responsiveness in the quiet animals was a little greater than that of the excitable group but by an amount so small as to be unreliable. Some dogs were consistently slower or faster than others. In two instances there were slight trends apparent, but they were in opposite directions. There was no general tendency for the latency to increase or decrease within either group. In fact, the major finding of the present study with regard to

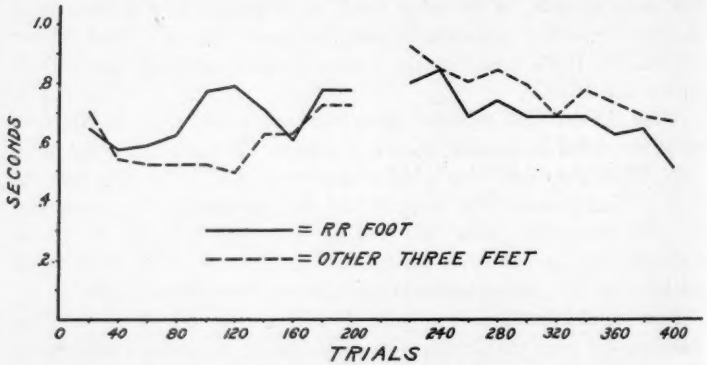

FIG. 8. COMPOSITE CURVES OF THE LATENCY OF CRS FOR ALL *S*s
The first half of these curves is based upon the data from 6 animals. The last half is based upon the data from five animals.

latency is its extremely small change throughout a fairly long period of training.

Fig. 8, in which the latencies are combined for all the *S*s, illustrates this fact. The values plotted represent the average latency for all CRs which occurred, for which unquestionable measurements to the nearest 0.1 sec. could be made. The last 200 trials as indicated in Fig. 8, show some slight evidence of a decreasing latency in the responses of all four feet, but this is largely counterbalanced by the increase which occurs within the first 200 trials. It will further be noted (for the last 200 trials) that the shocked member was lifted on the average a little earlier than the non-shocked members. Here again, however, the results are no more than suggestive and the actual differences are very small. The general constancy of the latency values obtained in this experiment is of greater significance than these minor variations. Its special interest lies in the fact that the

study of the latency of the CRs during training, as reported in recent experiments, presents a conflicting and somewhat obscure picture.

Hilgard and his collaborators, for example, have found a general decrease in latency with training, yet an analysis of their data shows that this relationship does not hold for all the *Ss*.[16] Wickens has reported that 'involuntary' conditioning is negatively correlated with frequency, while 'voluntary' conditioning may be positively correlated.[17] Unfortunately, however, the correlation coefficient which he obtained for his 'voluntary' group was not statistically significant. In an earlier publication from the present laboratory, Kellogg and Walker suggested that latency is on the whole negatively related to the stage of training, but their data were so irregular as to leave room for a large element of error.[18] The comparative unreliability of latency as a measure of performance in the conditioning situation was discussed at that time.[19] From the further analysis of the latency of the CR in the present experiment, it can only be concluded that the *average* latency may vary little throughout a long conditioning series.

Considered as a whole, these somewhat confusing results from different laboratories point to the generalization that latency as thus far measured, must certainly be regarded as the most unpredictable of the three common measures of the CR (frequency, amplitude, and latency).

SUMMARY

This experiment represents an attempt to get at the genesis of the process of conditioning in a measurable or quantitative way. The flexion CR of the right rear foot of dogs was the response conditioned, but kymographic records were made of the reactions of all four feet and of the respiration throughout the entire experiment. By this method a better picture of the general behavior of the organism was obtained than is possible when records are taken from the conditioned member alone.

Six unselected mongrel dogs were used as *Ss*. Five of them received 400 conditioning trials, and the sixth 200. The conditioned stimulus was a 1000 ~ buzz, 50-55 db. above the human threshold, which was sounded for 2 sec. The uncondi-

[16] E. R. Hilgard, Conditioned eyelid reactions to a light stimulus based on the reflex wink to sound, *Psychol. Monog.*, 41, 1931, (no. 184), 1-50; E. R. Hilgard and D. G. Marquis, Acquisition, extinction, and retention of the conditioned lid response to light in dogs, *J. Comp. Psychol.*, 19, 1935, 29-58; E. R. Hilgard and A. A. Campbell, The course of acquisition and retention of conditioned eyelid responses in man, *J. Exper. Psychol.*, 19, 1936, 227-247.

[17] D. D. Wickens, A study of voluntary and involuntary finger conditioning, *J. Exper. Psychol.*, 25, 1939, 127-140.

[18] W. N. Kellogg and E. L. Walker, An analysis of the bilateral transfer of conditioning in dogs, in terms of the frequency, amplitude, and latency of the responses, *J. Gen. Psychol.*, 18, 1938, 253-265.

[19] *Ibid.*, 261.

tioned stimulus was a D.C. shock of 0.2-sec. duration, delivered to the right rear foot at the termination of the buzz. A CR was credited to any limb which was lifted during the period of the conditioned stimulus.

(1) One or another of the *Ss* made CRs in all four feet at once, in three, two, or in any one of the feet alone, during the course of the training.

(2) The frequency of CRs in the shocked feet increased with training for the group as a whole, while the frequency of the CRs in the non-shocked feet decreased with training.

(3) Since the responses of the non-shocked feet were superfluous, unnecessary, or 'random,' their elimination is not unlike the elimination of errors in the typical trial-and-error experiment.

(4) Learning curves of the three non-shocked feet show how the *Ss* learned *what not to do*. The learning curve of the shocked foot shows how the *Ss* learned *what to do*.

(5) Those limbs which were anatomically closer to the point of electrical stimulation responded with greater frequency than the more distant parts. Thus the order of frequency of the CRs was RR, LR, RF, and LF.

(6) An examination of the individual records of the separate *Ss* shows that three were active or excitable while the remaining three were considerably more quiet during training. The data from the 'excitable' and the 'quiet' animals were therefore grouped together for comparative purposes.

(7) The excitable *Ss* made CRs in all four feet with a frequency of almost 100% from the start of training. Their learning problem reduced itself primarily to the elimination of the unnecessary or 'random' movements. They had to learn *what not to do*.

(8) The quiet *Ss*, on the other hand, made very few responses with any of the feet at the start of training. They had to learn *what to do* by building up a CR in the RR member. The learning curve for the foot conditioned in the case of these *Ss* possessed the typical S-shaped contour which is characteristic of conditioning.

(9) The conditioned flexion-response cannot be regarded as an adaptive reaction which is refined from out of the original raw material of generalized conditioning. Such a description will apply only to some cases. It does not hold for all.

(10) A treatment of results both in terms of frequency and amplitude of CR has supported this generalization. Yet frequency and amplitude as used in the present experiment are independent measures.

(11) The latency of the CRs from both shocked and non-shocked feet was roughly constant throughout the whole period of training. This is of special significance in view of the fact that some students of conditioning have reported a decreasing latency with training while others have reported irregular or inconsistent trends.

CEREBRAL MECHANISMS IN CONDITIONING UNDER CURARE

By EDWARD GIRDEN, Brooklyn College

It has been long believed that the effect of curare is to leave the organism functionally intact except at the myoneural junction. This would mean that the sensory, cerebral, and motor components of the nervous system are still capable of normal activity, but that neural innervation of the striate musculature can no longer be effected. One could thus attempt to establish conditioned responses (CRs)[1] during this drugged condition (life being maintained by artificial respiration) and test for modifications in behavior subsequent to recovery. Such a study was reported by Harlow and Stagner and the data interpreted in favor of a motor theory of learning.[2] In short, many combinations of bell followed by shock in the curare-state failed to establish a conditioned response of striate musculature. On the other hand, pupillary conditioned reflexes could be established under the drug which persisted undisturbed afterwards.

More recently, Girden and Culler have shown that these findings are equivocal for several reasons, and do not actually provide a crucial test of the motor theory.[3] Other phenomena also appeared which have led to several new considerations. It was found that, with doses of curare large enough to produce the usual symptoms of paralysis, a single isolated muscle such as the M. semitendinosus is still capable of reponding to *sensory* stimulation. This curare-response which then results is decidedly unlike the normal reaction, and it is possible that not only is there a quantitative reduction in the magnitude of the response under the drug but also concomitant qualitative changes.[4]

* Accepted for publication September 25, 1939. From the Psychological Laboratory, Brooklyn College. Special aid from the Penrose Fund of the American Philosophical Society and the Elizabeth Thompson Science Fund, which made possible this research, is gratefully acknowledged. A preliminary report of these findings was made at the meeting of the American Association for the Advancement of Science (Section I), Columbus, Ohio, December 28, 1939.

[1] The following abbreviations are used in this report: CR = conditioned response; CS = conditioned, substitute stimulus (bell); LSM = left semitendinosus muscle; RSM = right semitendinosus muscle. Successive curare and normal periods are indicated respectively by *1st Curare, 1st Normal, 2nd Curare,* etc.

[2] H. F. Harlow and R. Stagner, Effects of complete striate muscle paralysis upon the learning process, *J. Exper. Psychol.*, 16, 1933, 283-294.

[3] E. Girden and E. Culler, Conditioned responses in curarized striate muscle in dogs, *J. Comp. Psychol.*, 23, 1937, 261-274.

[4] There is independent evidence to show that, contrary to the traditionally accepted

These modified responses were capable of being conditioned to a bell while the animal was in the drugged-condition, and were evidenced *so long as and only when* the animal was in the curare-state. When the animal recovered from the drug, the CR was inhibited. That it had not been subject to extinction in the interim was manifest, for upon re-curarization the CR reappeared undiminished in strength. Tests made at the time indicated that there was a cortical inhibition under the drug, which was interpreted to mean: Curare CR = Thalamic CR; whereas Normal CR = Cortical CR. This hypothesis was of course applied only to the particular experimental situation which employed the use of striate musculature. Culler and his associates have since made more exact electrical measurements at the motor cortex and the spinal motor roots to determine the threshold sensitivity for the semitendinosus muscle for both normal and curare conditions.[5] Their results support the tentative hypothesis suggested by the previous functional data, and Culler concludes that "Somewhere between cortex and ventral root passes a plane of cleavage: a surface whose existence is demonstrated by our data even though its location and contour cannot be definitely stated. To one side of the boundary (cephalad) is a zone of depression; to the other (caudad) is a non-depressed (possibly sensitized) area."[6]

If, therefore, curare produces a condition of *functional decortication,* we have two systems capable of independent modifications. Whenever the cortical system is active, the subcortical is inhibited; if the cortical system is depressed, the subcortical pathways may be evoked.[7] If this hypothesis is correct, then *actual* decortication by operation should provide a situation in which this reciprocal amnesic condition can no longer be obtained. In such preparations the Curare CRs should appear in the normal state, and learning developed in the non-curarized situation should manifest itself in the drugged animal. The curare, however, may also inhibit the thalamic level, and, as suggested by Culler, "it is not impossible that

---

view, curare produces physiologically changes besides the block at the myoneural junction (cf. J. S. Light and W. H. Gantt, Essential part of reflex arc for establishment of conditioned reflex, *J. Comp. Psychol.,* 21, 1936, 19-36). In fact, the effect of curare upon the nervous system is not restricted to the motor-end-plate (cf. W. B. Cannon and A. Rosenblueth, The transmission of impulses through a sympathetic ganglion, *Amer. J. Physiol.,* 119, 1937, 221-235).

[5] E. Culler, J. D. Coakley, P. S. Shurrager, and H. W. Ades, Differential effects of curare upon higher and lower levels of the central nervous system, this JOURNAL, 52, 1939, 266-273

[6] *Ibid.,* 272 f.

[7] A somewhat analogous condition occurs in the effect of suggestion upon pupillary CR in human subjects, in which conscious 'sets' modify the unconscious pupillary CR. These data will be reported in a separate paper.

the plane of cleavage passes somewhere *below* the thalamus."[8] Negative results with decorticate animals would, therefore, be inconclusive, whereas positive findings would support the original hypothesis.

Specimens prepared for conditioning of the semitendinosus muscle can be used only for a few days. The decortication must therefore be performed first, with a sufficient period of post-operative rest to permit the recovery of function. What degree of decortication should be used? In order to explore the question systematically, the simplest situation was obtained for the present investigation; namely, bilateral extirpation of the cortical auditory areas.

All auditory conditioned reflexes (involving paw-flexion) are abolished when the sylvian gyri are removed bilaterally in a single operation.[9] Auditory function, however, has been attributed to the surrounding area.[10] Both the sylvian (areas 78 and 78b)[11] and the large surrounding ectosylvian gyri (areas 68, 68b, and 38b) were, therefore, removed completely from both hemispheres in a single operation. We can thus be certain that any subsequently established auditory conditioned responses must be subcortical in pathway so far as the conditioned stimulus (bell) is concerned.

Procedure. The cortical auditory areas were removed in a single aseptic operation performed under Nembutal anesthesia. After a lapse of at least two weeks the semitendinosus muscles were then exposed under ether anesthesia. After recovery curare was injected intravenously. Rarely was more than a minute or two required for the injection of the drug and the placing of the animal in the experimental cradle for testing or training. Either before or immediately after injection of the curare, a tracheal catheter was inserted.[12] Artificial respiration was maintained by means of a Harvard respirator. This process of curarization and recovery could be repeated indefinitely without mortality.

The time for complete recuperation is manifestly a function of a number of factors. In the first study, young animals, practically puppies, were used. In the present study, the dogs were from 1–2 yr. of age and weighed from 10–15 lb. The potency of different samples of curare is unpredictable.[13] In the present inquiry,

[8] Culler, Coakley, Shurrager, and Ades, *op. cit.*, 273.

[9] E. Girden, Cerebral determinants of auditory localization, this JOURNAL, 52, 1939, 1-15.

[10] Cf. J. W. Papez, *Comparative Neurology*, 1929, 1-518. Also F. Bremer and R. S. Dow (The cerebral acoustic area of the cat, *J. Neurophysiol.*, 2, 1939, 308-309) who have shown that the cerebral acoustic area in cats includes parts of the ectosylvian as well as the sylvian gyri.

[11] After Papez, *op. cit.*

[12] A brass tube, fit snugly over that end of the catheter which rested in the animal's mouth, protected the catheter from being bitten during the process of recovery. It was secured to the lower jaw by bandage, which helped keep the throat and mouth clear of the large quantities of saliva secreted during the drugged condition. The animal could thus be transported without fear of displacing the catheter.

[13] The artificial preparation of the active ingredient in the laboratory may eliminate

complete recovery occurred about two hours after the intravenous injection of approximately 1 cc. of a 1% solution.

The technique of conditioning the animals has already been described.[14] A bell was sounded for about 3 sec., terminating with the simultaneous application of a momentary shock to the paw of the desired hind limb. Animals 15 and 16 were trained under curare and tested after return to normal. Animal 17 was first conditioned in the normal state and tested in the subsequent drugged condition. After the animals were dispatched, the brains were placed in 10% formalin solution for further study.

Results. Animal 15. Twenty days after the cortical operation, both semitendinosus muscles were exposed under anesthesia, the catheter inserted, and the animal permitted to recover from the ether.

1st curare. The drug was injected at 3.00 P.M., artificial respiration initiated and the animal placed in the test-cradle. Conditioning scores with bell-shock combinations were as follows: 0/5, 0/5, 3/5, 3/6.[15] When incipient signs of recovery appeared, training was halted (3.40 P.M.). The response noted was the typical curare CR. Complete recovery occurred by 5.00 P.M. and artificial respiration was then halted, and the catheter removed.

1st normal. At 6.00 P.M. the animal was returned to the cradle and four tests with bell alone were made. (On all test-trials, the CS was given *alone* without shock reënforcement.) On the second trial there was general diffuse struggle behavior, as a consequence of which both semitendinosus muscles contracted. No responses whatsoever were made on the other three trials, the animal remaining completely inert. The reaction on the second trial was therefore interpreted to be the result of spontaneous activity and not conditioning to the bell.

2nd curare. After injection of the drug at 6.19 P.M. several preliminary tests with CSs alone failed to evoke the CRs. Conditioning was then continued until 7.00 P.M. with the following results: 3/5, 3/5, 5/5, 5/5, 5/5. The animal appeared to be even slightly deeper under the drug than in the first session.

2nd normal. Tests for the CR after recovery were made at 8.11 P.M. The following are the protocols of successive trials with CS alone:

(1) Both semitendinosus muscles contract; the left semitendinosus muscle (LSM) reacted first; then the right semitendinosus muscle (RSM). These reactions were followed by diffuse behavior. (2) A small twitch of LSM was followed by a slight movement of the rear part of the body. (3) Definite 'pull' as distinguished from 'twitch,' occurred in LSM. This started at the tip and spread through the body of the muscle, persisting for a second or two. (4) CR was the same as in the third trial, although the response was not as strong. (5) Again CR of LSM appeared, but limited this time to tip of muscle. (6) LSM CR this time was more like the characteristic curare-twitch.

3rd curare. The injection was made at 8.27 P.M. and four tests with CSs produced the following results.

---

this difficulty. Cf. K. Folkers and R. T. Major, Isolation of erythroidine, an alkaloid of curare action, from erythrina americana mill, *J. Amer. Chem. Soc.*, 59, 1937, 1580.

[14] Girden and Culler, *op. cit.*, 262 ff.

[15] The numerators denote the number of CRs, whereas the denominators indicate the total number of trials. In this session there were a total of 6 CRs out of 21 trials.

(1) CR of LSM was followed by a similar action of RSM, in typical curare-action. (2) CR again appeared as before. (3) Response in RSM was sharp, followed by a twitch of the tip of LSM. (4) Repetition of CR as noted in third trial.

The CR did not appear in the first normal period apparently because of insufficient training in the previous curare session. This interpretation is supported by the fact that the CR failed to carry over from the first to the second curare sessions. With more training in the latter, the CR now appeared in the subsequent, *i.e.* the second, normal period. Furthermore, the CR now persisted into the third curare session without further training. During the second normal period, there was some spontaneous activity which was not unusual under these circumstances. Great care was taken to avoid successive presentations of the CSs in a regular time-order. One could also anticipate the periods of activity and avoid test trials during these intervals.

While it is true that no tests preliminary to conditioning were made to determine the nature of the unconditioned reactions to the bell, we do know that the end result was true conditioning. The CR in the second normal session can be attributed only to the training in the previous, second, curare period, since there was no CR (or unconditioned response to the bell-alone) in the first normal session. As shown with the next two animals, the bell before conditioning produces no action of the semitendinosus muscles.

Animal 16. Twenty-five days after the cortical extirpation, the animal was taken to the experimental situation. Both semitendinosus muscles were exposed and the animal placed in the test cradle. The following protocols of 5 preliminary trials with CSs alone are relevant.

(1) A fine "shiver" occurred, but no discernable action of the semitendinosus, or any other muscles, noted. (2) This time the animal remained completely inert. (3) A little movement of the left hind limb-as-a-whole occurred. This produced a slight contraction of the LSM. (4) Left hind limb begins to twitch spasmodically before the bell was sounded. This action continued undisturbed while the CS was sounded. Neither semitendinosus muscle contracted. (5) Same as on previous trial.

Throughout the above test there was a continuous, slight shivering of the body (fine muscle-contractions) not unusual under the circumstances. The behavior during the third trial can be best understood as a natural consequence of exposure of the muscles, since a similar action occurred on the fourth trial before the CS was presented. The animal was surprisingly quiet in the situation.

1st curare. Some spontaneous contractions of the semitendinosus muscles, indicative of the curare-effect, appeared after curarization at 2.37 P.M. With the animal relaxed, two trials with CSs were negative; no reactions of these muscles

were evoked. CR training was now initiated with the following results: 0/5, 2/5, 3/5, 3/5, 5/5. The period was terminated at 3.05 P.M.

1st normal. Entirely recovered at 4.00 P.M. the test was given in the usual fashion with successive trials of CSs alone. The CR was shown to be unmistakably present as the following protocols reveal.

(1) Immediate contraction of LSM was followed by a general body twitch. (2) Separate contractions of LSM and a sharp jerk of the left hind limb distinct from the single muscle action were produced. (3) Less intense but clearly observed, was a twitch of LSM. (4) Delayed contraction of LSM appeared about ½ sec. before termination of the CS. The form of the response was this time less of a twitch and more of a 'slow pull.'

The presence of a general body tremor tended somewhat to disguise the CR of LSM. Every indication, however, pointed to the conclusion that these reactions were not artifacts but real conditioned responses to the bell. The test was completed at 4.20 P.M.

2nd curare. Under the drug at 4.23 P.M. the CR was again present. Further training was given, however, to reënforce the CR: 6/10, 6/10, 7/10, 6/10, 9/10, 10/12. Training was halted at 5.04 P.M.

2nd normal. At 6.14 P.M. the animal was back in the cradle, entirely recovered and ready for testing. The results of successive well-spaced trials with CS alone were as follows.

(1) Some possible contraction of RSM occurred. General body tremor was inhibited by CS. (2) Same as first trial, but head was also raised to CS. (3) Clearcut CR of LSM appeared about ½ sec. before the bell was terminated. Body tremor previously noted was again present undisturbed before, during and after the CS-interval. (4) CR of LSM again occurred as in the previous trial; body tremors, this time, possibly reduced by CS. (5) There was a possible CR of LSM; otherwise the results were much like the first trial. (6) Definite CR of LSM occurred with possible accompanying reaction of RSM. (7) No responses of any kind observed.

3rd curare. Tests with CS alone under the drug at 6.26 P.M. showed that the CR was still present, although somewhat reduced in magnitude.

We thus see that no original, unconditioned reactions of the semitendinosus muscles were produced by the bell in the untrained animal. After conditioning under curare, the CRs appeared in the subsequent first and second normal sessions. That there was some tendency for the CRs to become weaker is not surprising. A total of 77 training-combinations (bell-shock) had been applied during the first two curare sessions. After the second curare period no further training was given, although a total of 15 presentations of bell alone (without shock) were made in the two normal sessions and the final curare period. Clearly, the reduction in the magnitude of the CRs was a function of extinction.

Animal 17. Fourteen days after the cortical operation, the animal was taken to the test cradle. (2.10 P.M.).

1st normal. Five preliminary trials with CS alone were negative. On the first trial, the head oriented from left to right, but the rest of the body remained

inert. On the last 4 trials no reaction whatsoever could be detected. The animal was now given 100 combinations of bell-shock to left hind paw, which constituted the first conditioning session. On the following day, two more training periods were had, 125 and 165 trials respectively. By the end of the third session (concluded at 5.35 P.M.) the CRs in this intact left hind limb had developed to 100% level of performance. The semitendinosus muscles were now exposed. An additional 110 combinations of bell-shock to left hind paw were presented (6.35–7.42 P.M.) to insure satisfactory retention of the CRs in the exposed LSM.

1st curare. Drug was administered at 7.45 P.M. and the usual procedure was followed. Preliminary application of shock to paws of respective hind limbs produced typical unconditioned curare-twitch reactions. The critical tests with CS alone therefore begun at once, and the following observations were recorded.

(1) Both LSM and RSM contracted in "jiggle" fashion; that is, several successive jerks occurred simultaneously in both muscles. (2) There was a CR of LSM alone, as in the first trial. A single test with shock-alone to left hind paw was interposed at this time. The unconditioned response of LSM was now much weaker than in the preliminary tests made before the two bell-trials. (3, 4) Two more tests with bell-alone were now negative. Shock-alone again applied failed, this time, to produce any muscle-reaction. (5) In a little while, the muscle-response to shock-alone reappeared. Shortly after, the bell-alone evoked the CR of LSM (8.04 P.M.), somewhat limited in magnitude and restricted to a small section of the muscle. (6) At 8.06 P.M. CR of LSM was again produced by bell-alone. This time the response was full strength, typical curare-twitch, as noted before the interval of depression. On this last trial there also followed a weaker CR of RSM. At 8.25 P.M. the first symptoms of recovery were manifested: stronger heart action, and some occasional breathing reflexes.

Successive trials had been well spaced and presented in irregular time-order. No activity occurred, other than that reported above. Even the 'ripple' effects, noted in some animals under curare, were absent. It is clear that the failure of the CRs to appear on two of the trials with CS was due to a transition period of deeper curarization. When this happens, a rest must be taken until the return of curare-function is indicated by the reappearance of the shock-response. Once this occurred, the CR to bell returned full strength. Aside from the two negative trials during the interval of depression, the CR to bell was 4/4 (100%).

In the present animal we have the converse condition obtained in the previous specimens, Animals 15 and 16. In the latter two cases the CRs developed under curare persisted through the following normal period. In the present instance, Animal 17, the CRs established in the normal animal persisted in the following curare session. None of these results can be attributed to incipient recovery. In Animal 17 the striate musculature were still completely inert, and the first signs of recovery did not appear until at least 20 minutes after the conclusion of the test. In no case were tests under the drug ever continued until voluntary striate muscle activity appeared. To the contrary, testing was always halted just

as soon as any signs of recovery occurred. Even under such circumstances normal CR can not be developed.[16]

Discussion. It is clear from the present results that conditioned responses of the semitendinosus muscle to an auditory stimulus in the normal animal are mediated by cortical pathways, whereas the CRs developed under curare involves subcortical systems. When a section of the cortical pathway is literally extirpated, the block between the normal and curare states is disrupted; the CRs established in either one of the two conditions will now be manifested in the other state.

Some interesting questions now arise. Will the amnesia also be disrupted if the motor area is removed? Will this also happen if the somesthetic sensory area (essential to the conduction of the sensory impulses produced by the shock-stimulus) is extirpated? Can this reciprocal block be broken instead by the removal of cortical areas *not directly concerned* in this learning? Is the conditioning under curare completely subcortical in locus? Although the cortical system (auditory area) for the CS (bell) was extirpated in the present study, it is possible that the motor component was still, somehow, cortical in locus. Totally decorticate dogs are capable of developing conditioned responses[17] as are spinal preparations.[18] Can curare have a similar dissociative effect in totally decorticate animals, whereby a block is established between the thalamic (now the 'normal') level and some still lower brain center? All these questions are easily subject to experimental test.

Another point deserves consideration. It is accepted that these dissociations do not occur in the case of the pupillary CRs. However developed, in normal or curare state, they will appear in the other situation.[19] This fact does not contradict our hypothesis. Since Marquis and Hilgard have shown that extirpation of the occipital lobes does not disturb previously established visual conditioned lid responses, the latter were obviously subcortical in locus.[20] When the pathways for the CRs are cortical (as in the case of auditory paw-flexion conditionings) extirpation at this level

---

[16] H. Harlow (The effect of curare on the learning process, *Psychol. Bull.*, 34, 1937, 744) writes "Even when bodily movements were not entirely abolished the capacity for formation of conditioned responses [in the intact animal] was lost."

[17] E. Girden, F. A. Mettler, G. Finch, and E. Culler, Conditioned responses in a decorticate dog to acoustic, thermal and tactile stimulation, *J. Comp. Psychol.*, 21 1936, 367-385.

[18] P. S. Shurrager and E. A. Culler, Phenomena allied to conditioning in the spinal dog, *Amer. J. Physiol.*, 123, 1938, 186-187.

[19] Cf. Harlow and Stagner, *op. cit.*

[20] D. G. Marquis and E. R. Hilgard, Conditioned lid responses to light in dogs after removal of the visual cortex, *J. Comp. Psychol.*, 22, 1936, 157-178.

(cortical auditory areas) disrupts the CRs.[21] Curarization would, therefore, have no more effect upon pupillary conditioned reflexes than it does upon auditory CRs developed after extirpation of the cortical auditory areas. (Observation of the pupillary CRs in the intact animal is possible since the functioning of smooth muscle is unaffected by curare.)

Culler has suggested that the curare produces a condition analogous to "dual personality."[22] This term is usually applied to more complicated abnormal conditions in human beings, disturbances involving changes in character traits. A more appropriate analogy, that could be defended operationally, is the concept herein employed—*amnesia.* While the term dual personality was applied only in a descriptive fashion without any explanatory implication, the term amnesia is less susceptible to misuse or misinterpretation.

Intact animals conditioned under curare give every indication after recovery of having been unconscious.[23] It is extremely difficult to believe that conscious processes are involved in the development of conditioned reflexes in spinal preparations.[24] The interpretation of conditioning in totally decorticate animals is more difficult. Granting the presence of conscious behavior in the latter condition, not even the slightest indication of such activity is manifested in normal animals after recovery from curare. There is perhaps no value to be derived from raising this question of conscious behavior. However, in so far as the striate musculature is involved (which would also serve to express autonomic upset) these animals gave absolutely no indication of being aware of what had happened in the curare state. In this sense of the word, they were totally unconscious at the time.

The present data have some bearing on this question. In the partial decorticate, the amnesic condition is disrupted so that the curare CR now appears in the subsequent normal period. But the animal still appears to be unaware of what had occurred under the drug. That is, the animal does not manifest the diffuse struggle behavior ordinarily present during the first sages of conditioning.[25] Some of the protocols do indicate strug-

[21] Cf. Girden, *op. cit.,* 1-15; and Girden, Mettler, Finch, and Culler, *op. cit.,* 372 f.

[22] Culler, *op. cit.,* 267.

[23] This interpretation is supported by our own previous results as well as the data of the other investigators. The experimental technique, conditioning under the drug, and post-curare behavior have now been recorded on a 16-mm. film, entitled "The Dissociative Effects of Curare."

[24] Shurrager and Culler, *op. cit.,* 186 f.

[25] Cf. E. Girden, Conditioning and problem-solving behavior, this JOURNAL, 51, 1938, 677-678. On the other hand, the absence of such behavior may have some bearing on the question of the motor theory of learning.

gle behavior, but it is hard to say whether this is spontaneous activity or not. If it is a manifestation of diffuse CR behavior, its magnitude and frequency is far below the degree normally present.

It seems as if the animal is still unaware of what had happened under the drug, the semitendinosus muscle CR occurring much like the motor automatisms reported in hysterics. Whether the curare state is one of complete unconsciousness or the recovery period represents a condition of amnesia, is difficult to determine without introspective evidence. It does, however, seem possible, by studying specimens with varying degrees of decortication, to determine whether total organized (conscious?) bodily responses can eventually be produced in a post-curare situation after training under the drug. It would be highly desirable, of course, to make use of photographic techniques, and test for possible internal autonomic changes as well.

ACTIVITY OF THE HUMAN NON-PREGNANT UTERUS

By EDMUND JACOBSON, JULIUS E. LACKNER, and M. B. SINYKIN, Chicago, Illinois

Aside from pregnancy, very little is known concerning the activity of the human uterus. Bags for the registration of contractions during labor were first introduced by Schatz in 1872;[1] but in the non-pregnant human uterus the first mechanical study of contractions was made by Knaus, employing a manometer in 1929 and a bag in 1933.[2] In 1934, Moir working under conditions which he considered more comfortable for the patient concludes that "the investigations thus far have shown that it is always possible to record contractions in the non-pregnant uterus."[3] Bags have been used in recent investigations by one of us, particularly with reference to effects of hormones.[4]

The discovery that electricity shifts when striped muscle contracts was made by Matteucci almost one hundred years ago.[5] He placed two wires in a frog's muscle along the length of the fibers. His galvanometer showed that the wire closest to the point of contraction became for the instant negative, as compared with the other wire. Countless investigators have confirmed this finding. In 1907 Piper applied it to man.[6] In 1930 one of us showed how even microscopic contraction in human skeletal muscle can be measured quantitatively in electrical terms.[7] These methods are now applicable to the human uterus.

---

* Accepted for publication October 25, 1939. Presented before the Chicago Gynecological Society, May 19, 1939. The generous coöperation of the Bell Telephone Laboratories is gratefully acknowledged. From the Laboratory of Clinical Physiology, Chicago, and the Department of Gynecology and Obstetrics of Michael Reese Hospital.

[1] F. Schatz, Beiträge zur physiologischen Geburtskunde, *Arch. f. Gyn.*, 3, 1872, 58-144.

[2] H. Knaus, Eine neue Methode zur Bestimmung des Ovulationstermines, *Zentbl. f. Gyn.*, 53, 1929, 2193-2203; G. K. F. Schultze, Die Reaktion der nicht schwangeren menschlichen Gebärmutter auf Hypophysenhinterlappenextrakte, *ibid.*, 55, 1931, 3042-3052; H. Knaus, Die periodische Frucht- und Unfruchtbarkeit des Weibes, *ibid.*, 57, 1933, 1393-1408.

[3] C. Moir, Recording the contractions of the human pregnant and non-pregnant uterus, *Edinburgh Med. J. Trans. Obst. Soc.*, 93, 1934, 93-120.

[4] J. E. Lackner, L. Krohn, and S. Soskin, The etiology and treatment of primary dysmenorrhea, *Amer. J. Obst. & Gyn.*, 34, 1937, 248-265; F. H. Falls, J. E. Lackner, and L. Krohn, Effect of progestin and estrogenic substance on human uterine contractions, *J. Amer. Med. Assn.*, 106, 1936, 271-274; L. Krohn, J. E. Lackner, and S. Soskin, The effect of ovarian harmones on the human (nonpuerperal) uterus, *Amer. J. Obst. & Gyn.*, 34, 1937, 379-391.

[5] Ch. Matteucci, Sur le courant électrique des muscles des animaux vivants ou récemment tués, *Compt. rend. de l'acad. sci.*, 16, 1843, 197-214.

[6] H. Piper, Über den willkürlichen Muskeltetanus, *Arch. f. d. ges. Physiol.*, 119, 1907, 301-338.

[7] E. Jacobson, Electrical measurements of neuromuscular states during mental ac-

The first studies of electrical changes upon contraction in smooth muscle were made also in the laboratory of duBois Reymond, where Matteucci worked. Until the work of Fuchs (1910),[8] unstriped muscles were, however, generally supposed to be electrically inactive. Four years later, Foá noted a series of electrical waves indicating the discontinuous, tetanic kind of contraction in the pregnant uterus of the guinea pig.[9] He claimed that no electrical action-currents accompany certain spontaneous contractions. We challenge this claim of Foá since he used only a string galvanometer, the kind currently used in electrocardiography, which is not sufficiently sensitive to rule out the presence of low but significant action-potentials.

Uterine contraction-voltages have been studied in the guinea pig, cat, rat, and other mammals.[10] Most often the investigator has been interested in the electrical phenomena in their relation to the contraction of smooth-muscle fibers rather than as a means of recording the activity of the uterus considered as an organ. We shall not exhaustively review here the growing literature on electrical potentials in smooth muscle, including the uterus, but certain salient points should be mentioned. Authors who have registered both the electrical and mechanical phenomena during spontaneous activity agree in stressing the approximately simultaneous occurrence of the two processes (Orbeli and Brücke, 1910;[11] Alvarez and Mahoney, 1922;[12] Funke, 1922;[13] Richter, 1924;[14] Hasama, 1930;[15] Vozza, 1933[16]). When studying the electrical responses of smooth muscle in the uterus and other organs in cats, elicited upon stimulating the sympathetic nerves, however, Rosenblueth and co-workers (1933) distinguish an initial from a delayed response.[17] The initial response precedes the contraction, with a latency sometimes on the order of 1/10 sec.; the delayed response occurs during contraction, with a latency of the order of ca. 1/3-1/2 sec. In the uterus (pregnant) only the 'delayed' potential was identifiable. The authors believe that the initial potential is strictly comparable to the spike in the 'action current' of

---

tivities: IV. Evidence of contraction of specific muscles during imagination, *Amer. J. Physiol.*, 95, 1930, 703-712.

[8] R. F. Fuchs, Die elektrischen Erscheinungen am glatten Muskel, *Arch. f. d. ges. Physiol.*, 136, 1910, 65-100.

[9] C. Foá, Elektrophysiologie der glatten Muskeln, *Zentbl. f. Physiol.*, (Ergänzhft.) 27, 1914, 319.

[10] The customary term is 'action-potentials.' We proposed instead "contraction-voltages' for the potentials in electrodes in muscle-tissue associated with contraction.

[11] L. Orbeli and E. T. von Brücke, (1) Beiträge zur Physiologie der autonom innervierten Muskulatur; (2) Die Aktionsströme der Uretermuskulatur während des Ablaufes spontaner Wellen, *Arch. f. d. ges. Physiol.*, 133, 1910, 341-364.

[12] W. C. Alvarez and L. J. Mahoney, Action currents in stomach and intestine, *Amer. J. Physiol.*, 58, 1922, 476-493.

[13] H. H. Funke, Courants d'action des muscles lisses. (1) Électrogastrogrammes et mécanogastrogrammes de Rana esculenta, *Arch. néerl. de physiol.*, 6, 1922, 198-220.

[14] C. P. Richter, Action currents from the stomach, *Amer. J. Physiol.*, 67, 1924, 612-633.

[15] Bun-ichi Hasama, Pharmakologische Studien über den bioelektrischen Strom am Uterus; zur Registrierung des normalen Aktionsstrombildes des Uterus, *Arch. f. exp. Path. u. Pharmak.*, 153, 1930, 129-139.

[16] F. Vozza, Richerche sull'elettrometrogramma, *Ann. di ostet. e gin.*, 55, 1933, 603-615.

[17] A. Rosenblueth, C. Leese, and E. Lambert, Electrical potentials in smooth muscle, *Amer. J. Physiol.*, 103, 1933, 659-680.

skeletal muscle which precedes contraction. Their failure to obtain it in the uterus is due, as they are inclined to think, to a voltage so small as not to be revealed by the apparatus employed. If so, we can assume that such initial potentials are included in the records to be presented here, because the apparatus which we have used is unique in its sensitiveness. We shall, however, make no attempt to distinguish possible 'initial' from 'delayed' potentials, for our interest is in the action of the uterus regarded as an organic whole.

According to Bozler (1938) the musculature of the uterus is, like that of the heart, a syncytium.[18] Using long thin strips of uterine muscle from guinea pigs, rabbits and cats, he demonstrates 'all-or-none' contractions following various electrical stimuli. In short, he takes issue with Rosenblueth and associates, for he finds that the electrical changes during a conducted response differ only quantitatively from those observed in nerve and striated muscle.

Aside from slight temporal differences, there is much evidence, as we have said, that the mechanical and electrical phenomena occur in parallel. Most particularly, Fletcher (1937) has found in the smooth muscle of a certain mollusk (the byssus of Mytilus edulis) proof that the parallelism there is quantitative.[19]

Contraction-voltages in the human uterus have been somewhat crudely studied by several investigators. The electrocardiograph was used by Theilhaber in a study of uterine hemorrhage in 1910.[20] One electrode was applied to the cervix; the other was inserted in the rectum up to the level of the fundus. He believed that he found many typical rises in the curve in younger women without abnormal bleeding. These rises were absent when the uterus had been extirpated. It seems safe to say, in the light of our own experience, that no electrode should be in the rectum and that some of the waves which Theilhaber recorded were probably picked up from the cervix, at least in part. Veit (1912) stated briefly that he obtained records of uterine contraction from the feet, but he does not indicate their character.[21] Bode (1931) also used the electrocardiograph,[22] but says nothing about electrodes; he was interested in the action of posterior pituitary hormone on the pregnant uterus. With the same type of apparatus, Falk and Nahon (1935) placed a curved silver rod in the uterine cavity, presumably touching the mucosa.[23] To test the theory that the non-pregnant human uterus shows spontaneous contractions, the authors undertook to secure electro-uterograms periodically throughout the menstrual cycle. They admit that the origin of the waves they secure is somewhat problematical. We agree; for an electrode in the uterine cavity exposed to chemical action of tissue-fluids may show potentials in consequence. In any event, silver is not a metal to use with any degree of safety in this respect.

---

[18] E. Bozler, Electric stimulation and conduction of excitation in smooth muscle, *ibid.*, 122, 1938, 614-623.

[19] C. M. Fletcher, Relation between mechanical and electrical activity of molluscan unstriated muscle, *J. Physiol.*, 91, 1937, 172-185.

[20] A. Theilhaber, Insufficientia Uteri, atonia Uteri, hyperplasia Uteri und Uterusblutungen, *Monatsch. f. Geburtsh. u. Gyn.*, 31, 1910, 727-740.

[21] J. Veit, Das Elektrometrogramm, *Zentbl. f. Gyn.*, 36, 1912, 161-162.

[22] O. Bode, Die Wirkung des Hypophysenhinterlappenhormons auf den schwangeren Uterus im Elektrohysterogram, *ibid.*, 55, 1931, 2887.

[23] H. C. Falk and R. Nahon, Electro-uterography, *Amer. J. Obst. & Gyn.*, 30, 1935, 403-408.

Through the use of thermionic amplification, extremely sensitive methods for the study of uterine contraction voltages are now open to the physiological psychologist. Our attempts to make an introductory survey concerning the electrical and mechanical activity of the human non-pregnant uterus have been partially described; but it is believed that a résumé, along with a fuller account of other studies, may prove of interest.

Methods. In many of our experiments, contraction-voltages were recorded from the uterus and from the abdominal striated muscles simultaneously.

For this purpose, two string galvanometers were available. The string's tension was set at 3-4 mm. per mv. in order to secure a flat characteristic for frequencies up to 200 cycles at least. By means of a switchboard any one of the three amplifiers could be thrown in circuit with either of the galvanometers. These assemblies have been described previously by Jacobson,[24] but modifications as here employed have not. Amplifier-1 is of the resistance-capacity type, and was most often used in the study of the uterus because of its ability to record lower as well as higher frequencies. It lacks the voltage sensitivity of Amplifier-2 and of Amplifier-3. The latter two are a twin pair, transformer-coupled, capable of recording higher frequencies from about 30 to 4000 with a flat characteristic. At the maximum, we have employed them in the present study to secure an over-all sensitivity a little less than 1 cm. per μv, a.c. Accordingly it is possible to secure amplification about 1000 times that of the string galvanometer alone.

Electrodes should penetrate directly into the uterine tissue, if chance variations of heat are to be averted. We, therefore, use platinum-iridium wires with points sharpened for easy penetration as described previously by Jacobson.[25] The wire is pushed into the tissue of the cervix or fundus about 5 mm. up to a hilt of insulating rubber or bakelite. Concentric electrodes were employed in one study only; namely, on a virgin dog's uterus. We preferred platinum-wire electrodes in order to record more extensively from muscle fibers than would be possible with the concentric type.

Pneumatic records were procured by means of a balloon in the uterine cavity. The passing of air from within this balloon took place through a rubber tube connected with a modified Marey tambour. Movements of the tambour facing were conducted by means of an inelastic thread to an extremely delicate system.[26] In effect, the passage of small amounts of air into or out of the balloon was recorded by means of a lever moving freely so that its position made a shadow on the photographic record. By-pass tubes with stop-cock arrangements made it possible to inflate the balloon after it was in the uterus, using an air-syringe; while the pressure in the system could be tested with an oil-manometer. As a rule, when used most

[24] E. Jacobson, Electrical measurements of neuromuscular states during mental activities. I. Imagination of movement involving skeletal muscle, *Amer. J. Physiol.*, 91, 1930, 567-608; VII. Imagination, recollection and abstract thinking involving the speech musculature, *ibid.*, 97, 1931, 200-209.

[25] Jacobson, Measurement of the action-potentials in the peripheral nerves of man without anesthetic, *Proc. Soc. Exp. Biol. & Med.*, 30, 1933, 713-715.

[26] Cf. Jacobson, Electrical measurements of neuromuscular states during mental activities. IV. Evidence of contraction of specific muscles during imagination, *Amer. J. Physiol.*, 95, 1930, 703-712.

sensitively, the pressure was approximately equal to a column of water 95 cm. in height.

The patient lies on an extremely comfortable couch in a shielded room adjacent to the space which contains the galvanometers and most of the other instruments. Amplifiers and standardizing devices are concealed in the couch. Both the leads and the air-tubes pass from *S* to connections through the wall. In this manner *S* is generally kept relatively free from disturbance through movement and speech of the experimenters.

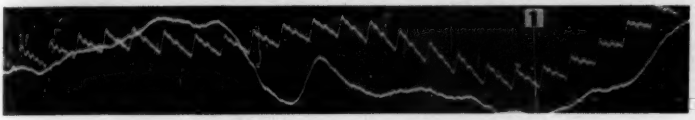

FIG. 1. (EKU-B) A UTERINE CONTRACTION-CYCLE RECORDED ELECTRICALLY (LOWER CURVE ON RIGHT) AND MECHANICALLY

Microvoltage for the cycle ca. 300. The control test (input of amplifier short-circuited) is almost a straight line (not shown). Deflection is upward when the electrode in corpus tissue becomes electronegative; 1 cm. = 65 μv. Upward deflection of mechanical tracing denotes uterine contraction. Time-unit (left lower corner) = 0.2 sec.

Electrodes were inserted into the uterus in three types of location; (a) both in the corpus tissue; (b) one in the corpus and the other in the cervix tissue; and (c) both in the cervix, anterior or posterior lip. The corresponding records are called Electro-uterograms, Type A, B, and C respectively, and may be abbreviated as EKU-A, EKU-B, and EKU-C. For our purposes, insertion of both electrodes in the cervix is the most convenient and is attended by the least discomfort.

In order to record entirely within the corpus (EKU-A), a modified tracheal catheter is employed. The platinum-iridium wires contained within were insulated with bakelite varnish throughout their length, excepting at the tips. They were freely movable longitudinally, and could be made to protrude from the end of the catheter to a distance determinable upon a scale marked in mm. on the wire near a metal connection which served as a handle for the device.

In securing tracings of Type EKU-B, we employed either a similar device containing one wire only or else a metal catheter insulated externally, but with a central tube for the passage of one wire surrounded by a passageway for air currents leading to and from the bag which was inserted into the uterine cavity. The central wire could be withdrawn into the tube during the insertion, but could be protruded when the instrument came into place.

For tracings of Type EKU-C, no special device was needed. The two platinum-iridium wires were inserted directly into the exposed lips of the cervix.

Results. Twenty-nine *S*s were available for the present study, of whom 15 were free from noteworthy pathology. The account contained in the present report will be limited chiefly to the findings in the relatively normal women.

Marked uterine contractions are recorded mechanically and electrically in Fig. 1. What appears to be a complete cycle of contraction is shown. The duration in this

instance is approximately 20 sec.; though the time varies widely, even in successive contractions. This *S* was a multipara of 24 yr., who complained of 'nervousness' prior to menstrual periods. She mentioned also occasional pain in the lower portion of the abdomen bilaterally passing to the legs intermittently since her last delivery. Her uterus was movable, but retroverted and was held in proper position by a pessary. This recording was made about three weeks after her previous period and two weeks before the following. Tracings made on the same occasion from the skeletal muscles of the abdomen near the mid-line changed from a level of about 1 μv. at first (maximal peak; *i.e.* the voltage of the maximal action-potential recorded per unit of time, 0.2 sec.), then to 6, and later to 12 during a recording period of about 12 min. It might be concluded that this *S* was in a state of neuromuscular hypertension during this period,[27] which coincided with her complaint of premenstrual nervousness and, to a certain extent, objectively confirmed that complaint.

Measurements of the states of activity in skeletal muscles, denoting whether the individual appears to be in a hypertensive or in a relatively normal condition at the time, are made in μv. However, it is well to emphasize that there is no hard and fast line between the normal and the nervous hypertensive patient expressible in these units. Indeed, the actual microvoltage determined in a particular muscle at a particular moment will vary with the type of amplifier as well as with the character, position, and dimensions of the electrodes employed. Under such variable conditions, prolonged experience with the amplifier and the electrode-assembly employed is pre-requisite for reliable interpretation.

Less marked uterine contractions are traced electrically in Fig. 2a. This *S* likewise complained of 'nervousness' prior to menstrual periods. At times for three years there had been also some pain in the bladder and lower portion of the abdomen. She was a multipara of 45 yr. with irregular menses. The last period had ended six weeks previously. The left ovary and tube, as well as part of the cervix, had been removed in 1930. A small fibroid tumor was palpated in the right cornu of her uterus.

This case affords another opportunity to determine objectively the nervous state of the patient—at least at the time of the test. Fig. 2a affords a brief example of the sort of moderate contraction-voltages recorded at irregular intervals, often less than 1 sec. Fig. 2b clearly shows that when a separate set of electrodes is inserted into the (skeletal) abdominal muscle, a record is secured of contraction-voltages, which is independent of the uterine record (2a). The uterine voltages shown in Fig. 2a are not very high. Similarly the records of the abdominal neuromuscular state show no excessive voltages such as one of us has shown commonly can be recorded during highly nervous states.[28] Our *S* contracts and relaxes the abdominal muscles promptly upon signal, a performance which is not characteristic of individuals at the time of high emotion. Accordingly we find in this instance no objective record of nervous hypertension, either in the abdominal skeletal muscles or in the uterus. Such negative findings suggest the doubt that this patient was really in a state of 'premenstrual nervousness' at the time of the study.

---

[27] Jacobson, *Progressive Relaxation,* (rev. ed.), 1938, 6-27, 309-326.

[28] Jacobson, Electrical measurements concerning muscular contraction (tonus) and the cultivation of relation in man Studies on arm flexors, *Amer. J. Physiol.*, 107, 1934, 230-248.

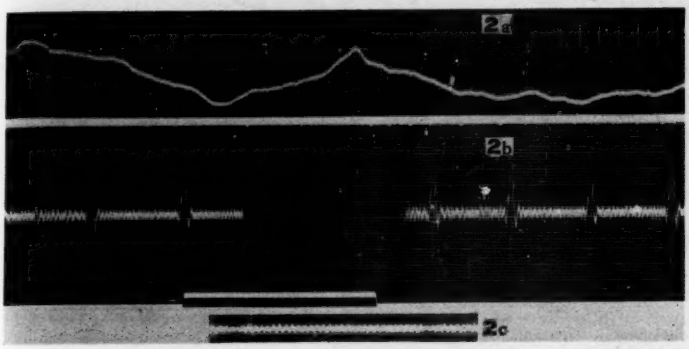

FIG. 2a. (EKU-B) Moderate contraction-voltages. Signal-line at top of record marks patient pulling in abdominal muscles as requested. No great influence on uterine contraction-potentials is shown; but when later repeated, continuously for 6 sec., the electrical deflections showed increased amplitude, lasting some seconds after the abdominal muscle-contractions ceased. The short-circuit recording is an almost straight line (not shown). 1 cm. = 75 μv. (Deflection is upward when the electrode in corpus tissue becomes electropositive.)

FIG. 2b. Record of contraction-voltages in two electrodes inserted in abdominal muscles several cm. apart, about 5-6 cm. above pubic hair. Note the regularly recurring electrocardiogram. Note also that otherwise the abdominal muscles show low contraction-voltages, except upon signal (bottom of record). While *S* contracts these muscles upon request, contraction-voltages increase, corresponding in time with the signal-mark in Fig. 2a. *S* relaxes promptly upon a second signal-sound (end of the signal-line). 3.5 mm. = 1 μv.

FIG. 2c. Short-circuit control test. Compare with Fig. 2b.

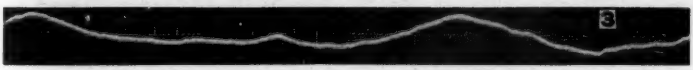

FIG. 3. (EKU-C) Records similar to EKU-B, except that the contraction-voltages are lower. 1 cm. = 75 μv. Time lines = 0.2 sec.

From the same *S* a record is presented (Fig. 3) to illustrate that contraction-voltages can be recorded most conveniently with both electrodes in the lips of the cervix. Curves sometimes resemble those secured with one electrode in the body of the uterus and one in the cervix (Type EKU-B). The voltages, however, are lower in the case of Type EKU-C.

Fig. 4 presents records from a fourth *S* who, regarded from a clinical standpoint, was obviously of 'nervous' type. Her age was 38 yr., she had borne two children, and had suffered one miscarriage. The record was made on the 21st day of the menstrual cycle (a 28-day rhythm). *S* complained of pain in the lower abdomen and of vaginal discharge; the uterus was retroflexed and slightly enlarged, and there was a 'second-degree' prolapse.

The electrical recordings of Fig. 4a indicate marked contractions in the uterine

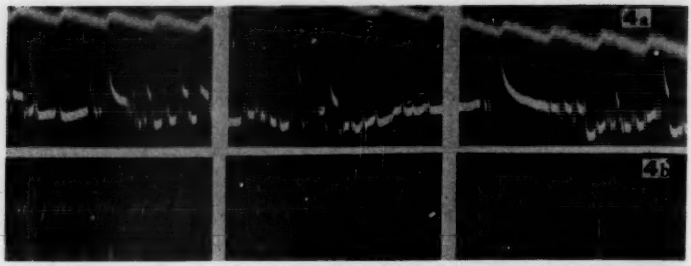

FIG. 4a. The upper tracing is a mechanical record, the lower the EKU-B in a 'nervous' *S*. Each vertical interspace indicates a lapse of ca. 18 sec. The entire record represents ca. 1 min. In the mechanical tracing appear rhythmic variations due to arterial pulsation. Pressure in the air system is ca. 95 cm. of water. Upward deflection signifies uterine contractions (notable in the middle part of record). The electrical tracing, like the mechanical, indicates marked uterine contractions. 1 cm. = 100 μv. The control test (short-circuiting the input terminals) is approx. a straight line, indicating that the results are not artifacts.

FIG. 4b. Contraction-voltages in abdominal skeletal muscles, occurring approx. simultaneously with the tracings shown in Fig. 4a. 3.5 mm. = 1 μv. Electrocardiograms barely discernible. Short-circuit control test similar to Fig. 2c.

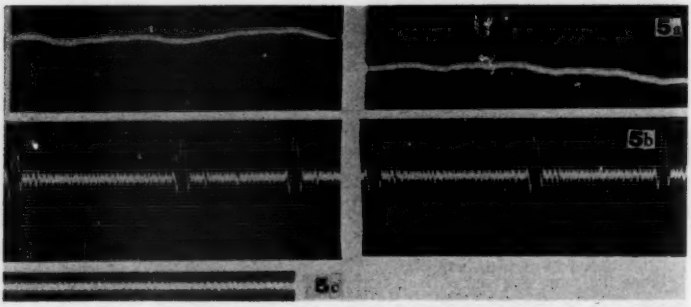

FIG. 5a. (EKU-B) In the 17-sec. interval between the two photographs (white space), the negative potential in the electrode in the fundus declines about 20 μv.(= 1 cm.). Shallow wavelets are superimposed ca. ½ — 2 per sec. The uterus shows a low or moderate activity.

FIG. 5b. Electrodes in abdominal wall, 4 cm. below umbilical level, each about 2.5 cm. from midline. Six complexes of cardiac origin appear. Between these complexes the string tracing is fairly quiet. Comparing such intervening sections with Fig. 5c, the abdominal muscles here recorded are almost completely relaxed (cf. Fig. 5c). Records 5a and 5b are taken at approximately the same time.

FIG. 5c. Control test, showing slight fluctuation of the string tracing when a short-circuit exists across the input of the amplifier. 1 μv, a.c. = 7 mm.

musculature.[29] A similar interpretation can be made from the mechanical (upper) tracings. At the same time, contraction-potentials from the abdominal musculature (Fig. 4b) indicate that this *S*, for the moment at least, was in a somewhat tense state.

By way of a contrasting illustration, recordings from another *S* are shown in Fig. 5 *S* (age 25 yr.) ended her first pregnancy six weeks previously and had been nursing her child. The uterus was undergoing normal involution. When the electrodes were inserted in the cervix, the EKU-C illustrated continual tonic contraction (not here shown). (Tonus in the intact human uterus had not been electrically recorded prior to the present studies.)

Further records from the same *S* illustrated that at times, at least, tracings of

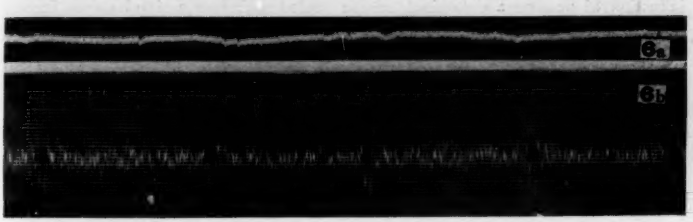

FIG. 6a. (EKU-B) Connections and *S* same as for Fig. 2a.

FIG. 6b. Connections and *S* same as for Fig. 2b. Electrodes in the abdominal muscles evince a peak microvoltage of about 6 or 7 μv. between heart-beats, whereas in Fig. 2b their microvoltage was approximately zero. The uterine musculature as recorded in Fig. 6a, however, is more quiet than in Fig. 2a.

Type EKU-C are similar in general character to those secured by the more difficult methods, EKU-A and EKU-B.

Fig. 5 illustrates a slight or moderate degree of uterine contraction, as electrically recorded (5a), coincident with approximately complete abdominal muscle relaxation (5b). With right fist vigorously and persistently clenched for some seconds, contraction-potentials increased markedly in the abdominal musculature. *S* failed to relax upon signal, but rather became more tense, illustrating something found by one of us in previous studies; namely, that many persons require training to relax before they are prepared to do so effectively upon signal or on their own intent.[30] During one period of such failure to relax tensions in the abdominal skeletal muscles, contraction-potentials in the uterine musculature increased at first but later proceeded to decrease, although the abdominal tension was still increasing. That the uterus can on occasion become relatively quiet even while the skeletal abdominal muscles are relatively tense is illustrated in Fig. 6, from this same *S*.

---

[29] Artifacts in the tracings of Type EKU-A and EKU-B cannot be ruled out with certainty since it is impossible to determine with complete assurance that an electrode in the corpus has not slipped out at any moment. In this respect, tracings of EKU-C are more dependable, since it is possible at the end of the experiment to determine the location of the electrodes by direct inspection.

[30] Jacobson, Electrical measurements concerning muscular contraction (tonus) and the cultivation of relaxation in man; Relaxation-times of individuals, *Amer. J. Physiol.*, 108, 1934, 573-580.

If the figures presented above indicate that uterine contraction-potentials proceed with rhythm and magnitudes not predictable from the examination of skeletal muscles alone, *e.g.* the abdominal muscles, we are not warranted in drawing the conclusion that the magnitude of abdominal muscle-contraction fails to influence that of the uterine musculature. A careful study of the relationship between such magnitudes in the smooth and skeletal muscles involved would require a systematic study of this problem alone and would require also the use of *Ss* trained to relax previously or possessed of such natural ability. In the absence of thorough-going studies, we leave open the question of the influence of the abdominal-musculature tension upon uterine contraction. In some of our studies it seemed evident that the induction of persistent abdominal tension tended to induce a simultaneous increase in uterine contraction, while, in other instances, no such relationship was clearly observed.

Significant is the fact that it is now possible to study objectively the nervous patient with pelvic symptoms, since electrical tracings can now be made simultaneously from the skeletal and the uterine musculature.[21]

SUMMARY

(1) A method is set forth for a study of the activity, in pathological and in normal states, of the intact uterus *in situ*.

(2) An additional procedure has been described for the measurement of skeletal neuromuscular states. A combination of these methods makes it possible to secure an objective record of the 'tense' or 'nervous' state of an individual during a particular test in conjunction with a synchronous record of the electrical and mechanical activity of the uterus.

(3) Uterine tonus, registered during the present investigation for the first time in the intact organ, varies considerably in magnitude from time to time and from *S* to *S*. Whether the uterus ever becomes entirely free from contraction could not here be determined because we had no *Ss* trained to relax.

(4) Spontaneous contractions were recorded repeatedly from the uterus, even with no bag in the fundus. Accordingly, the objection sometimes raised to conclusions from previous investigations that spontaneous contractions were characteristically due to the presence of a foreign body is not confirmed. We cannot, however, confirm or deny Moir's statement that the normal uterus always shows spontaneous contractions. This problem would require a larger series of cases, employing at least a number of individuals previously trained to relax.

(5) The uterus was recorded as active or quiet whether or not the ab-

[21] Cf. E. Jacobson, J. E. Lackner, and M. B. Sinykin, Electrical and mechanical activity of the human non-pregnant uterus, *Amer. J. Obst. & Gyn.*, 38, 1939, 1008-1021.

dominal neuromusculature was relatively active or quiet. Although four combinations were found to occur, we discovered no conclusive evidence against the plausible view that conditions under which abdominal musculature is tense tend to promote uterine activity; what evidence we have on this subject would tend to favor this view. It is certain, however, that recorded tensions in the abdominal and uterine muscles do not always vary in correspondence.

STUDIES ON CONTOUR: STROBOSTEREOSCOPIC PHENOMENA

By HEINZ WERNER, Wayne County Training School

It has been demonstrated in an earlier study that under certain circumstances a solid form will not be seen if its presentation is immediately followed by that of a second figure.[1] In the demonstration of this phenomenon, two visual figures were used: an 'inner figure,' a solid black disk (15 mm. in diameter) on a white background; and a 'frame figure,' a black ring (inner diameter 15 mm.) on a white background. The black disk will not be seen providing (a) that the two figures are observed repeatedly in succession; and (b) that they are presented at an optimal rate of succession, *i.e.* the intervals between figures and between the pairs of figures are of a certain optimal length.

Since it is of theoretical significance whether this and allied phenomena can be obtained during binocular interaction, a strobostereoscopic experiment has been devised.

METHOD AND PROCEDURE

The strobostereoscope,[2] used previously in studying binocular depth perception, was adapted for the present study. The apparatus permits the projection of each figure to one eye only; for example, the ring to the left and the disk to the right eye.

RESULTS

Table I shows the dependence of the disappearance of the disk upon rate of succession. In this experiment the disk appeared always as the first member of the pair. The figures in the columns indicate, for each rate of succession, the percentages of statements of complete disappearance of the disk. The table indicates that the phenomenon of the disappearance of the disk—previously observed under ordinary visual conditions—continues to occur even when each eye is receiving only one of the alternating stimuli. It should be noted that the optimum rate of succession is different for different *Os*.

If the order of the succession is reversed so that the ring appears as the first member of the pair, the disk does not disappear within the limits of speed mentioned above. If black and white are reversed in the figures

* Accepted for publication December 14, 1939.

[1] Heinz Werner, Studies on contour: Qualitative analysis, this JOURNAL, 47, 1935, 40-64.

[2] Werner, Dynamics in binocular depth perception, *Psychol. Monog.*, 48, 1937, (no. 218), 1-127.

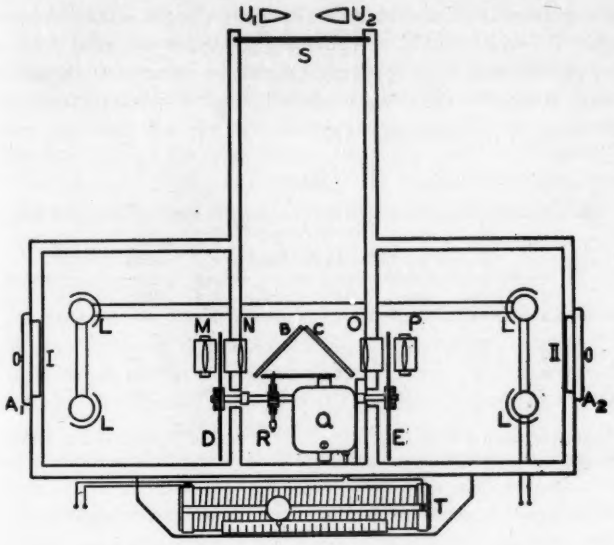

FIG. 1. STROBOSTEREOSCOPE

The Pictures I and II, held in the frames A_1 and A_2, after receiving illumination from the lamps (L), are projected through the lenses (M, N, and O, P) and are reflected from the mirrors (B and C) to the opaque glass (S). The projections are interrupted by episcotister disks (D and E). The disks are operated by means of motor (Q) and gear (R). The speed of rotation is varied by the rheostat (T). *O* looks through the curved prisms (U_1, U_2) at the opaque glass where the images appear.

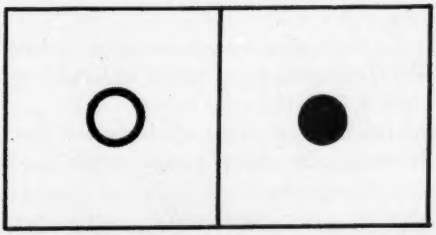

FIG. 2. FORMS USED IN THE STROBOSTEROSCOPIC EXPERIMENT

In the course of each trial the pair of figures is presented several times. On each square of opaque glass where the images will appear, is a tiny dot which serves as a fixation-point. The *O*s were trained to keep the two points constantly fused during every trial. Four *O*s took part in the experiment. There were 20 trials for each series.

the phenomenon is not altered. At the optimum rate the white disk becomes invisible, the white ring alone appearing at regular intervals.

The phenomenon of disappearance cannot be observed if the disk and the inner diameter of the ring are relatively small with respect to the width of the ring. If, for instance, the diameter of the disk is 2 mm, and the

TABLE I

PERCENTAGE OF DISAPPEARANCE OF DISK RELATIVE TO RATE OF SUCCESSION AND TIME OF ROTATION

O	Rate of succession (in ms.)* and time of rotation (in sec.)†								
	50 0.2	75 0.3	100 0.4	125 0.5	150 0.6	175 0.7	200 0.8	225 0.9	250 1.0
1	0	20	70	70	80	100	40	10	0
2	0	0	25	70	70	55	50	0	0
3	0	0	10	50	100	80	60	50	0
4	0	0	0	40	70	70	100	40	20

* Interval between disk and ring. † Interval between disk and disk

TABLE II

THE OPTIMAL EXPOSURE-TIME (IN MILLISECONDS) UNDER STROBOSTEREOSCOPIC AND ORDINARY BINOCULAR CONDITIONS

O	Optimal exposure-time (in ms.) under	
	strobostereoscopic conditions	binocular conditions
1	175	150
2	125–150	125
3	150	125
4	200	180

inner and outer diameter of the ring 2 and 15 mm respectively, the disk will remain visible.

A comparison of the optimal rate of succession under the strobostereoscopic conditions of the present experiment and under ordinary binocular conditions is given in Table II.

The optimum rate of succession at which the disk disappears is greater for all *O*s under strobostereoscopical conditions than in ordinary binocular vision.

DISCUSSION

The present study has demonstrated that the disappearance of a disk succeeded by a ring can be observed whether both figures are seen binocularly, or one figure is presented to the right, the other to the left eye. The facts brought out in this study seem to have significance for the general theory of interaction within the visual field.

About the time the first study on contour was published, two papers on apparently allied phenomena appeared. Fry exposed in a dark visual field a small bright white area *b*; after a lapse of time two adjacent areas *a* and *a'* were exposed. He determined the duration of exposure of *a* and *a'* necessary to produce a sensation of brightness equal to that produced by *b*.[3] Keeping the duration of exposure of *b* constant at 4 ms. he found that the time values of *a* and *a'* were considerably shorter than 4 ms., varying with the interval between the two exposures as follows.

Interval (in ms.)	7.5	15	25	50	75	100	125
Duration of *a* and *a'* (in ms.)	2.6	1.8	1.3	0.8	0.4	0.2	0.15

By this and other experiments Fry proved that the second flash has a depressive effect upon the first. In interpreting his results, Fry states, "What seems to happen is that the response of the retina to the first stimulus is considerably delayed and prolonged and overlaps in time the response to the second stimulus and is inhibited by it by some kind of interaction between retinocortical pathways at synapses either at the retina, or at the basal ganglia or at the cortex."[4] Piéron[5] published several experiments with forms similar to those which we used in Experiments 15 and 16 of the first study,[6] obtaining essentially the same effect as we have described. An oblong figure followed by two adjacent bands was more or less inhibited. Fry does not attempt to decide whether the interaction occurs at the peripheral or central level, whereas Piéron is inclined to interpret the inhibition as an interaction at the peripheral level, that is, in the retina.

Some of the facts published in our first study suggest, however, that higher integrating centers are involved in producing the phenomenon. It may be recalled that *O*'s attitude played an important rôle. The *O*s were able to enhance or to reduce the phenomenon intentionally. Most pertinent to the present question is Experiment 21 with ambiguous frame figures.[7] If the frame figure is perceived so that the inner figure fits into it, the latter will disappear. If *O* perceives the frame figure in such a way that no configurational relation exists between it and the inner figure, the latter continues to be seen. These results indicate that all the facts cannot be explained on the basis of Piéron's theory. They suggest that the phenomenon

[3] G. A. Fry, Depression of the activity aroused by a flash of light by applying a second flash immediately afterwards to adjacent areas of the retina, *Amer. J. Physiol.*, 108, 1934, 701-707.
[4] *Op. cit.*, 706.
[5] Henri Piéron, Le processus du métacontraste, *J. Psychol. Normale Pathol.*, 31, 1935, 5-24.
[6] *Op. cit.*, this JOURNAL, 51 ff.
[7] *Ibid.*, 55 ff.

is not based upon any point-by-point stimulation and interaction at the retinal level. Perceptual processes on a higher level of integration (*i.e.* contour processes) obviously play a fundamental rôle.

The results of the present study supply further evidence in support of this view. They demonstrate that the primary interaction cannot occur peripherally, since the strobostereoscopic phenomenon is based on interaction between different stimuli presented to the two retinal fields. Of course, the question could be raised whether one is not dealing here with a primitive phenomenon of binocular rivalry, of suppression of the activity of one eye in favor of the other.

It is true, as we have frequently observed, that binocular rivalry does occur not only under simultaneous stimulation but also under alternate stimulation of the two eyes. There are several facts, however, which seem to exclude the interpretation of the phenomenon in terms of binocular rivalry.

(1) If the ring succeeds the disk, rivalry may occur, but only at a rate of succession much greater than the optimum rate. Under these circumstances the inner figure is frequently visible.

(2) If suppression of one image in terms of binocular rivalry is the cause of our phenomenon, the order of presentation within the pair should have no effect on its occurrence. Actually, however, if the succession is reversed, that is, if the ring precedes the disk, the latter, as a rule, remains visible at the optimum rate.

(3) Again, suppression by rivalry does not explain why the disk does not vanish when its diameter is very small in relation to the width of the ring. On the contrary, one should expect that the disappearance of the disk would be even more easily and firmly established when the discrepancy between the two figures is greater.

(4) Finally, the *O*s frequently observe that the ring expands when it appears alone. This observation, previously noted under ordinary binocular conditions, seems to confirm our assumption that *O*'s response to the stimulus preceding the ring, though not on the level of form perception, is dynamically involved in the organization of the ring.

SUMMARY

A black disk and a black ring of the same inner diameter as the disk are projected strobostereoscopically, one to the left and one to the right eye. At an optimum rate of succession the disk disappears. This experimental observation supports the view that the phenomenon of depression or inhibition described in the first study on contour, does not originate at the retinal level but is the result of a perceptual process at a higher level of integration.

DISTRIBUTION OF EFFORT IN MEMORIZING PROSE AND POETRY

By ARTHUR P. BUMSTEAD, Berkeley, California

More than fifty years ago Ebbinghaus discovered that less effort was required for memorizing when units were small and when learning was spread or distributed. Though these results with nonsense syllables have since been confirmed by many investigators, there is no general agreement among psychologists regarding the most economical way of memorizing poetry or prose. Some psychologists contend that the greatest economy is secured when a passage to be memorized is

TABLE I

MILTON'S *Paradise Lost*: NUMBER OF READINGS, STUDY TIME, AND ELAPSED-TIME REQUIRED FOR LEARNING SELECTIONS OF DIFFERENT LENGTHS AT VARIOUS TIME-INTERVALS BETWEEN SINGLE READINGS

Measure	Length of selection in lines	Intervals between readings									
		0	1 hr.	2 hr.	4 hr.	8 hr.	12 hr.	1 d.	2 d.	4 d.	7–8 d.
No. of readings	350	—	—	—	—	—	—	—	—	—	16
	50	49	43	34	28	24	—	19	16	15	14
	25	29	—	—	—	—	21	—	17	—	—
	5	24	18	—	—	—	13	—	8.5	—	7
	1	—	5.3	—	—	—	—	—	4.5	—	3.5
Study-time (in min.)	350	—	—	—	—	—	—	—	—	—	400
	50	170	140	115	95	75	—	60	48	50	46
	25	47	—	—	—	—	29	—	23	—	—
	5	6	4	—	—	—	3	—	2	—	2
	1	—	1	—	—	—	—	—	1	—	1
Elapsed-time (in days)	350	—	—	—	—	—	—	—	—	—	112
	50	1	4	6	7	8	—	19	32	60	98
	25	1	—	—	—	—	11	—	34	—	—
	5	1	2	—	—	—	7	—	17	—	49
	1	—	1	—	—	—	—	—	9	—	25

broken into parts and learned by repeated readings over long intervals. They hold to this view because the actual learning or study-time in hours and minutes is short. Other psychologists, contrariwise, regard that method as wasteful because of the long time elapsing between the first and last reading.

How are study-time and elapsed-time affected by changes (1) in the length of the intervals between readings and (2) in the size of the units learned? In order to answer these questions specifically, the writer performed upon himself the experiments reported here.

About 1000 lines from Milton's *Paradise Lost* were memorized at various times between 1915 and 1935, and about 1400 lines from the *Bible* during the years following. The results are shown in Tables I and II.

* Accepted for publication September 16, 1939.

Learning of a typical selection from *Paradise Lost* will be described in detail. One day S read a 50-line passage from beginning to end at his usual rate. He repeated the process every day until he could recite the selection to himself without referring to the text. After a few readings he could recall an isolated line here and there, and after a few more he could repeat sections up to 10 or 15 lines in length. He read the entire passage through every day, however, until he could recite the entire 50 lines. Table I indicates that the time spent in actual study of the text was 60 min. As there were 19 readings spaced one day apart the elapsed-time was 19 days.

Nine 50-line selections were memorized. The lengths of the intervals between single readings were variable, ranging from 0 to 8 days, but all other factors

TABLE II

THE *Bible*: NUMBER OF READINGS, STUDY TIME, AND ELAPSED-TIME REQUIRED FOR LEARNING SELECTIONS OF DIFFERENT LENGTHS AT VARIOUS TIME-INTERVALS BETWEEN SINGLE READINGS

Measure	Length of selection in lines	Intervals between readings	
		1 hr.	7 d.
No. of readings	1200	—	50
	200	30	10
Study-time (in min.)	1200	—	1085
	200	240	60
Elapsed-time (in days)	1200	—	350
	200	3	70

were held fairly constant. There was a pronounced tendency for decreased study-time and increased elapsed-time to be associated with decreases in the intervals between readings. For instance, 43 readings spaced 1-hr. apart (in the daytime) were needed for learning one passage. The study-time was 140 min. and the elapsed-time was 4 days. When a piece of the same length was learned by readings spaced 8 days apart the study-time was reduced from 140 min. to 46 min.; and the elapsed-time was increased from 4 days to 98 days. Similar results were obtained when the selections were 1 line, 5 lines, and 25 lines in length.

Selections from the *Bible* (Table II) were memorized by a slightly different procedure. The first 14 chapters of *Job*, comprising 1200 lines were memorized in 50 study-periods spaced 1 week apart. At the first period the chapters were read through once, the operation taking 35 min. After 10 readings, one at each period, the first two chapters, containing 200 lines, were written down on paper from memory; they were omitted from subsequent readings. After 9 more readings the third and fourth chapters, also containing 200 lines, were committed to memory; they too were recorded and omitted from later readings. As time went on more and more chapters or substantial parts of chapters were written down, and the learning periods became correspondingly shorter. The 50th period, which was the final one, was only 4 min. long.

The story of Samson and Delilah as related in the 16th chapter of *Judges* was memorized as a result of 30 readings spaced at 1-hr. intervals. After 5 readings it was noted that one of the 31 verses could be recalled; after the 10th

reading 5 verses, and after the 20th reading 17 verses could be repeated. After the 30th complete reading the whole chapter could be recited, and it was written down.

For learning 200 lines from *Job* by readings spaced one week apart the study-time was 1 hr., and the elapsed time was 70 days. For learning a comparable passage from *Judges* by readings spaced 1 hr. apart the study-time was 4 hr., and the elapsed-time was 3 days.

It must be pointed out that lines from the *Bible* and *Paradise Lost* are not comparable in length nor in difficulty.

If a selection were divided into parts which were learned consecutively, how would study-time and elapsed-time be affected? We saw in Table I that 23 min. of study-time were needed for learning a 25-line selection by readings spaced 2 days apart, and the elapsed-time was 34 days. It seemed reasonable to suppose that a second passage of the same length could be memorized by the same method in the next 34 days, and that the study-time would again be approximately 23 min. Then the study-time and elapsed-time for learning 50 lines divided into two parts of 25 lines would be respectively 46 min. and 68 days, comparing with 48 min. of study-time and 32 days of elapsed-time required when a 50-line selection was learned as a whole by readings every second day. Similarly, if a 50-line passage were divided into 10 parts of 5 lines which were learned consecutively the study-time would be reduced to 20 min. approximately, while the elapsed-time would be almost 6 mo.

When the experimenter had used the data in Tables I and II for making the computations in Table III he reached the conclusion that when a selection is divided into parts which are memorized consecutively, the smaller the parts the shorter will be the study-time and the longer will be the elapsed-time required for the learning.

If a selection is divided into parts which are memorized concurrently rather than consecutively, how are study-time and elapsed-time affected? In order to answer this question the writer memorized three more 200-line narratives from the *Bible. Shadrach and Company in the Burning Fiery Furnace* was divided into 2 parts of 100 lines each; *Samson's Riddle* was divided into 4 parts of 50 lines each; and *Belshazzar's Feast, or the Handwriting on the Wall,* was divided into 8 parts of 25 lines. There was always an interval of 1 hr. between any two readings of each part, and learning of all the parts of each narrative was under way at the same time.

Part A of the first of these narratives was read once an hour on the hour until it had been memorized and written on paper. Part B was learned concurrently, but the readings were on the half-hour. No part was read again after it had been recorded. The actual study-time for this narrative was about 2 hr. 25 min. The elapsed-time, disregarding periods devoted to sleep at night, was 24 hr. It is estimated that if the parts had been learned consecutively the elapsed-time would have been 48 hr., and the study-time would have been about 2 hr. 25 min. as before.

For the narrative divided into 4 parts, the study-time was 2 hr., and the elapsed-time was 20 hr. When the narrative was divided into 8 parts, the study-time was 1 hr. 25 min., and the elapsed-time was 15 hr. It will be recalled that

TABLE III

COMPARISON OF ACTUAL TIME REQUIRED FOR MEMORIZING PASSAGES AS A WHOLE WITH ESTIMATED TIME REQUIRED FOR MEMORIZING BY PARTS LEARNED CONSECUTIVELY

Interval between readings	Lines	Study-time (in min.)	Elapsed-time (in days)
0	50	170	1
	25	47	1
	2×25 (est.)	94	2
	5	6	1
	10× 5 (est.)	60	10
2 days	50	48	32
	25	23	34
	2×25 (est.)	46	68
	5	2	17
	10× 5 (est.)	20	170
7-8 days	50	46	98
	5	2	49
	10× 5 (est.)	20	490
7-8 days	350	400	112
	50	46	98
	7×50 (est.)	322	686
	5	2	49
	70× 5 (est.)	140	3430

when the story of Samson and Delilah was memorized as a whole by hourly readings, the study-time was 4 hr., and the elapsed-time was 30 hr.

Comparative results obtained from learning 200-line passages by various methods are summarized in Table IV, together with estimates of the time which would have been required if the parts had been learned consecutively.

TABLE IV

COMPARATIVE RESULTS OBTAINED BY LEARNING 200-LINE NARRATIVES AS A WHOLE; BY PARTS CONCURRENTLY; AND ESTIMATED, BY PARTS CONSECUTIVELY

(Each part was read at hourly intervals)

Method of Learning	Study-time (in hr.)	Elapsed-time (in hr.)
Learning as a whole (200 lines)	4.0	30.0
Learning in 2 parts of 100 lines, concurrently	2.4	24.0
Learning in 4 parts of 50 lines, concurrently	2.0	20.0
Learning in 8 parts of 25 lines, concurrently	1.4	15.0
Learning in 2 parts of 100 lines consecutively (est.)	2.4	48.0
Learning in 4 parts of 50 lines consecutively (est.)	2.0	60.0
Learning in 8 parts of 25 lines, consecutively (est.)	1.4	120.0

Table III may be interpreted to mean that when a passage is divided into parts which are memorized concurrently the smaller the parts the shorter will be the study-time and the shorter will be the elapsed-time.

Between 1915 and 1939 the writer memorized about 3000 lines from *Paradise Lost* and the *Bible*. The selections ranged in length from 1 line to 1200 lines; the intervals between single readings varied from 0 to 8 days; the study-periods

ranged in length from a few seconds to 35 min.; memorizing was by wholes, by parts learned consecutively, and by parts learned concurrently. He arrived at the following conclusions regarding his own memorizing when selections are learned by the method of repeated readings.

(1) The longer the interval between readings, the shorter the study-time and the longer the elapsed-time.

(2) When a passage is divided into parts which are learned consecutively, the smaller the parts the shorter the study-time and the longer the elapsed-time.

(3) When a passage is divided into parts which are learned concurrently, the smaller the parts the shorter the study-time and the shorter the elapsed-time.

ALTERNATIVE INTERPRETATIONS OF RESULTS OBTAINED ON A VARIATION OF THE PETERSON MENTAL MAZE

By C. W. CRANNELL, University of California

In most learning experiments the experimenter (*E*) sets a criterion of 'good' behavior on the part of his *S*s and scores all activity which deviates from this criterion as 'bad.' To be sure, *E* rarely, if ever, uses such terms as 'good' and 'bad,' in describing the behavior of his *S*s, but speaks instead of 'correct choices,' 'errors,' etc., defining these as objectively as possible. Such objective definitions arbitrarily make 'errors' out of all behavior which does not lead in a manner prescribed by *E* to a condition in which *S*'s responses coincide with the criterion set by *E*.

In the vast majority of animal experiments the situation is arranged so that trying out different responses is 'bad,' while sticking to those which have proved successful in a single instance is 'good.' For example, if a rat has avoided a given blind in a maze on one trial and enters it on the next, this response is scored as an error. It may be, of course, necessary to do so for the sake of an objective record. *E* should not forget, however, that he is scoring two very divergent types of behavior as if they were the same thing: (1) a 'bad error,' failure to retain the 'correct' response at that part of the maze; (2) a 'good error,' an attempt to discover a better way out of the maze.

It may be possible to obtain some knowledge of the presence of such differences and of their importance as factors in learning by studying set-ups in which doing different things is 'good' from *E*'s point of view. The experiment here reported seems to the writer to illustrate how *E*'s methods of treating the data and his establishment of a particular kind of criterion of performance may determine the results he reports.

The recent introduction of Woodworth's *Experimental Psychology,*[1] which contains a very complete account of Peterson's mental maze experiment,[2] as a text in courses at the University of California has made it advisable to devise a variation of the mental maze which could be used in the laboratory on *S*s with knowledge of the original form. It was decided to use alternate forms of the maze, with ten pairs of letters in each. In Maze I the pairs were P–Z, G–L, R–Q, A–V, B–X, T–E, H–N, D–U, Y–F, and J–M, with the first letter in each pair as written above correct. In Maze II there were the same pairs of letters in the same order, but with the second letter in each pair correct, making a sort of 'mirror image' of the first maze.

The lists of letters were written on the opposite sides of cards which were furnished to the student who served as *E*. The *S*s, other students, were given mimeographed instructions copied verbatim from Peterson's article and sent from the room to read them, while the *E*s were instructed as to the operation of the experiment.

* Accepted for publication January 5, 1940. This study was directed by Professor Warner Brown.

[1] R. S. Woodworth, *Experimental Psychology,* 1938, 153 f.

[2] J. Peterson, Learning when frequency and recency factors are negative, *J. Exper. Psychol.,* 5, 1922, 270-300.

E's instructions. The following instructions were given *E.* "You will begin with Maze I and present the first pair of letters, P–Z. Write down the letters in the order presented, P–Z if the correct letter is given first, and Z–P if the wrong one is given first. Correct or wrong first is to be determined by whether you draw a red card (correct first), or a black card (wrong first) from the deck which has been given you. Underline *S*'s response. If the response is Z, which is wrong, repeat the letters P–Z in the order as determined by drawing another card, and continue to do so until *S* responds with P. Then proceed to the pair, G–L. If *S* responds with G, proceed to R–Q, but if he responds with L *turn the card over* and start at the beginning of Maze II with Z–P. Now you have to present Z–P in random order as determined by the deck of cards until *S* says Z, then proceed to L–G, in which L is now correct and G leads back to the start of Maze I. Whenever *S* makes an error on either of the mazes, except at the first pair, P–Z or Z–P, you will always turn the card and start in again at the beginning of the other maze.

"When *S* finally makes the correct response on the last pair of letters, you are to say, 'That is the goal. Now you must run through the maze again.' You tell *S* how many errors he made on his first trial, by counting up the number of P–Z, Z–P pairs. *Then you turn the card to the other maze and begin the second trial.* You continue until *S* makes three perfect trials in succession. For example, on Maze II, then Maze I, and then on Maze II again."

The main features of the procedure as given in the instructions to the *E*s as outlined above are: (1) a return to the start of the alternate maze upon every error except an error on the first pair of letters; (2) a return to the start of the alternate maze upon having reached the goal in the first trial; and (3) the reaching of a criterion of three errorless trials in succession, the middle one of which would be on the form of the maze which was alternate to the other two criterion trials.

Twenty-one useable records were obtained after rejecting five because of failure to complete learning within the time allotted and several because of mistakes made by the *E*s.

It was thought by the writer that those *S*s who had achieved 'insight' in this problem would know that upon every return to the start the maze changes, while those who had merely learned a series of letters which somehow led farther, and had not grasped the principle of alternation would try to use the same responses over again. It might be reasoned that those *S*s who became cognizant of 'something queer' about the start and therefore varied their responses during learning would make the 'insightful' response upon starting the maze the second time, but those who ignored this feature of the maze would stick to their stereotyped solution.

Accordingly the *S*s were divided into two groups: 'insightful' *S*s, who, having reached the goal on one maze for the first time (*i.e.* completed trial 1), responded with the letter correct for the *other maze* on the first two pairs of letters on the second trial; and 'insightless' *S*s, who responded with the letter correct for the maze they had just finished.

Table I shows the results for these two groups of *S*s. The 'insightful' *S*s take much longer to reach the first errorless run (last 10 pairs of letters in trial 1), as measured by the number of pairs of letters presented. They take longer to reach the second errorless run, but are faster in reaching the start of the first criterion trial. In fact, ten of the 'insightful' *S*s made no more errors after reaching the goal for the first time (the remaining four *S*s progressed down the correct maze for from four to nine pairs of letters, then apparently failed to retain the correct response and made one error).

The columns, B, C and D, of Table I show how the activity of the *Ss* was distributed during the first trial. The 'insightful' *Ss* spent relatively more time in progression along the mazes than they did in fumbling with the first two pairs. 'Progression' is here defined as reaching the third pair of letters or beyond, while merely reaching the second pair is classified with the repetitive errors, since this pair

TABLE I

PAIRS OF LETTERS PRESENTED UP TO VARIOUS STAGES IN LEARNING

	S	Pairs to start of first errorless run (Trial 1)	Distribution of activity during first trial			Pairs to start of second errorless run (Trial 2)	Pairs to start of first criterion run
			Repetitive errors	Progression along			
				Alternate maze	Same maze		
		(A)	(B)	(C)	(D)	(E)	(F)
'Insightful'	1	338	174	78	86	348	338
	2	276	139	67	70	291	291
	3	244	107	87	50	254	244
	4	228	67	81	80	243	273
	5	197	91	60	46	207	197
	6	182	94	56	32	192	182
	7	173	58	55	60	183	173
	8	150	68	31	51	160	150
	9	141	57	6	78	189	189
	10	90	32	34	24	100	90
	11	88	16	45	27	98	88
	12	69	22	27	20	79	69
	13	67	13	28	26	77	67
	14	66	21	22	23	94	94
	Mean	164.9	68.4	48.4	48.2	181.1	174.6
'Insightless'	15	222	87	6	129	234	351
	16	161	93	32	36	192	252
	17	85	49	43	3	96	131
	18	72	35	0	37	92	216
	19	71	52	9	10	174	199
	20	50	28	10	12	85	124
	21	37	13	0	24	50	179
	Mean	97.1	51.0	14.3	35.9	131.9	207.7
Mean proportions			B/A	C/A	D/A		
'Insightful' *Ss*			0.378	0.314	0.308		
'Insightless' *Ss*			0.516	0.144	0.340		

had to be reached, and incorrectly responded to, if the *Ss* were fixated on using only one form of the maze. The 'insightless' *Ss* spent a greater proportion of time on repetitive errors than did the 'insightful' *Ss*, and they also spent relatively little time in progression along the alternate maze.

These scores were also changed into proportions of total pairs presented on trial 1. The means of these proportions are shown in Table 1 and appear to confirm the analysis above. Another method of checking this finding, the computation of

the mean length of a progression along the maze on which the goal was first reached as compared with the alternate maze, yielded a similar result.

Insofar as data from such small groups can be trusted, it would appear that those *Ss* who reacted 'correctly' to the arbitrary condition set by *E* for the second trial were those who were already aware of the existence of alternate routes through the maze. Introspective reports showed this to be the case. Those *Ss* who had selected only one possibility as a route were unable to make this "correct" response readily, nor did they seem to learn very much about the possibility of an alternate route during the second trial.

The present writer believes that the results are indicative of something more important than their intrinsic meaningfulness. When preparing the set-up for use in the laboratory the writer predicted that those *Ss* who first caught on to the alternate maze aspect (*i.e.* who showed 'insightful' behavior early in learning) would be the first to reach the goal. The results obtained show that the reverse is true. If, instead of reversing the maze for the start of the second trial, the *Ss* had been allowed to use the same side, it seems obvious, although not yet demonstrated experimentally, that the 'single-track' *Ss* would have completed the experiment much sooner than those who paid attention to both routes. Theirs would then have been considered the 'good' performance.

It is only by the addition of an arbitrary condition of 'insightful' performance, which the writer chooses to regard as a 'good' performance, that the possibility of alternative interpretations of the results becomes evident. It would seem that not only the development of apparatus and procedure, but also the criterion of success—and therefore, to a great extent, the results of many learning experiments,—may be dependent upon the theoretical bias and valuative concepts of the *E*.

THE FORGETTING OF PLEASANT AND UNPLEASANT EXPERIENCES

By L. I. O'KELLY, University of Colorado, and
L. C. STECKLE, Denison University

The comparative memory-value of pleasant and unpleasant experiences has repeatedly been investigated.[1] The *raison d'être* of the present study is to report the results obtained over longer recall-periods than have previously been used. Stagner used a period of 15 days,[2] Meltzer of 6 weeks,[3] and the present study a period of 10 weeks.

The results reported here were obtained from 96 *Ss*—about equally divided as to sex. All were students in elementary psychology. On the first day of classes following the Christmas recess (*i.e.* on January 5, 1939), the *Ss* were asked to write all the pleasant and unpleasant experiences they had had during the holidays. One group was instructed to list the pleasant experiences and then when this was done, to list the unpleasant ones. The order of affective listing was reversed for the second group. Ten weeks later, without warning, the groups were again asked to recall the pleasant and unpleasant events of the Christmas vacation.

In the tabulation and comparison of the results of the two recall-periods, the events reported at the second but not at the first recall were omitted from consideration.

The 96 *Ss* reported a total of 1179 experiences at the first recall. Of these, 741 (62%) were pleasant and 438 (37%) were unpleasant.[4]

The second recall, 10 weeks later, gave a total of 529 experiences which had been recalled before. Apparently 650 (55%) of the originally reported events had been forgotten.[5] Of the 529 remembered experiences, 354 (67%) were labeled pleasant, and 175 (33%) unpleasant. After 10 weeks then, 387 (52%) of the original pleasant events and 263 (60%) of the original unpleasant events

* Accepted for publication October 3, 1939. This study was made at Ohio State University during January and March, 1939.

[1] H. Meltzer, The present status of experimental studies on the relationship of feeling to memory, *Psychol. Rev.*, 37, 1930, 124-139; Individual differences in forgetting pleasant and unpleasant experiences, *J. Educ. Psychol.*, 21, 1930, 399-409; A. T. Jersild, Memory for the pleasant as compared with the unpleasant, *J. Exper. Psychol.*, 14, 1931, 284-288; Ross Stagner, The redintegration of pleasant and unpleasant experiences, this JOURNAL, 43, 1931, 463-468; M. M. White and M. M. Ratliff, The relation of affective tone to learning and recalling words, *ibid.*, 46, 1934, 92-98; M. M. White, Some factors influencing recall of pleasant and unpleasant words, *ibid.*, 48, 1936, 134-139; M. M. Ratliff, The varying function of affectively toned olfactory, visual, and auditory cues in recall, *ibid.*, 51, 1938, 695-699.

[2] Stagner, *op. cit.*, this JOURNAL, 43, 1931, 465.

[3] Meltzer, *op. cit.*, *J. Educ. Psychol.*, 21, 1930, 401.

[4] Our results stand in remarkably close agreement with Meltzer's. He found that out of the total of 2231 experiences 1393 (62.4%) were pleasant and 838 (37.4%) were unpleasant. (*Op. cit.*, 402.)

[5] After 6 weeks Meltzer (*op. cit.*, 404) reports 46.9% forgotten.

had been forgotten. Or, conversely, 354 (48%) of the original pleasant events and 175 (40%) of the original unpleasant events were recalled.

The results in Table I (if they are comparable) indicate that the relative amount of pleasant and unpleasant experiences recalled tend to approach each other as time goes on, with pleasant events apparently being forgotten somewhat

TABLE I

PERCENTAGES OF *P* AND *U* EXPERIENCES RECALLED

	Original (this study)	Original (Meltzer)	15 days (Stagner)	6 weeks (Meltzer)	10 weeks (this study)
P	62	62.4	54.4	53.0	48
U	37	37.4	43.4	39.8	40

more rapidly than unpleasant. The difference at 10 weeks (8%) is, however, still significant, being ten times as large as its standard error (0.72).

An analysis of the individual results again showed the presence of the dichotomy, pessimist-optimist, reported by Meltzer.[6] Of the 96 *Ss*, 60 (62.5%) retained a greater proportion of pleasant than unpleasant experiences, 30 (31%) retained a greater proportion of unpleasant than pleasant experiences, while 6 (6%) were classified as indifferent, retaining an equal proportion of both affective divisions.

A further examination of these two classes gives other interesting results. An attempt was made to determine wherein lay the differences obtained between the pessimists and the optimists. The formulae, $P_1 - P_2$ and $U_1 - U_2$ were applied to every *S* in each of these groups. From this computation two columns of figures were obtained for each group (optimists and pessimists). These results appear in Table II.

TABLE II

AVERAGE EXPERIENCES FORGOTTEN BY OPTOMISTS AND PESSIMISTS

Pessimists		Optomists	
Mean P_1-P_2	4.8	Mean P_1-P_2	3.9
Mean U_1-U_2	1.4	Mean U_1-U_2	3.5
diff.	3.4	diff.	0.4
$\sigma_{diff.}$	0.5	$\sigma_{diff.}$	1.3
$D/\sigma_{diff.}$	6.8	$D/\sigma_{diff.}$	0.3

From this it would appear that while the optimists tended to forget both pleasant and unpleasant experiences about equally, the pessimists forgot pleasant events significantly more rapidly.

In order to investigate the possibility that the optimist-pessimist dichotomy might be a manifestation of one of the commonly recognized contrasted personality types (extravert-introvert, etc.), the Bernreuter Personality Inventory was given to all *Ss*.[7] Correlations between number of experiences recalled, both P and U, with the measures in the inventory, ranged from —0.01 to —0.27. This does not mean, of course, that no personality differences exist, characteristic of the pessimists and optimists, differentiated on statistical grounds. Rather, it would appear that our results

[6] *Ibid.*, 409.

[7] R. H. Bernreuter, *The Personality Inventory*, 1931.

suggest the possibility of the existence of another variable in personality measurement.

If the differential forgetting of pleasant and unpleasant experiences is due to an increased repression of the unpleasant, and not to a difference in the number of experiences of each type impressed, the results of this experiment indicate that only a part of the general experimental population evidences the repression. In this finding the results of Meltzer's study have been confirmed,[8] and their statistical reliability has been demonstrated.

[8] Meltzer, *op. cit.*, 407-409.

APPARATUS

TWO DEVICES FOR MEASURING DRIVING SPEED ON THE HIGHWAY

By C. H. LAWSHE, JR., Purdue University

With the increasing realization of the importance of the human element in traffic and highway safety, a growing need has been felt for methods by means of which the behavior of drivers can be measured. We early discovered, in a highway research undertaken in July, 1938, that the then existing methods and devices for measuring driving speed had their limitations. Though adequate for many purposes, most of them were subject to errors of reaction-time on the part of the investigator and were not easily adapted for use at night. Moreover, they required that the investigator station himself in positions in which he could be seen by the oncoming driver. The devices described here obviate these difficulties.[1] Both are oscillographs; they differ only in that one utilizes a cathode ray tube and the other an optical lever for recording the oscillogram.

THE CATHODE RAY RECORDER

The basic part of the cathode ray recorder is contained in an instrument case (Fig. 1 B), 10 x 10 x 27 in. It is composed of a cathode ray oscillograph, exposed and unexposed film-magazines, a motor and gear assembly for driving the photographic film, and an electrically driven tuning fork.

FIG. 1. THE CATHODE RAY RECORDER

'A' is the 6-v. storage battery; 'B,' the instrument case containing the oscillograph, tuning fork, and mechanism for driving the film; and 'C' is the combined data and control board. The wire (w) is the lead to the pressure switches.

[1] These devices were developed in a study sponsored by the Joint Highway Research Project and the Division of Education and Applied Psychology of Purdue University. The research was directed by Dr. Joseph Tiffin. For assistance my thanks are due Professors F. B. Knight of the Division of Education and Applied Psychology, R. H. George and H. J. Heim of the School of Electrical Engineering, H. M. Jacklin of the School of Mechanical Engineering, and M. B. Steer of the Speech Clinic.

Oscillograph. A type 913 (1 in.) cathode ray tube is used in an adaptation of the basic circuit (Fig. 3) with a Mallory No. 554 Vibra-Pak and a 6-v. storage battery power supply. Oscillations on the screen of the tube are obtained with an interrupted D.C. potential of approximately 90 v., on the vertical deflecting plates of the tube, produced by a 100 ~ electrically driven tuning fork in conjunction with an ordinary door-bell transformer used as a step-up.

Photographic record. A photographic record of the wave pattern is obtained by sweeping horizontally across the screen of the tube a strip of 35-mm. Eastman Recording Paper (No. 1) by means of a 6-v. horn motor which is controlled by

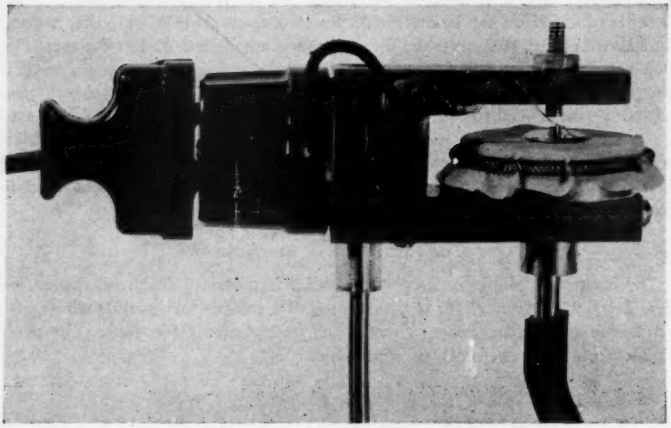

FIG. 2. THE TAMBOUR PRESSURE-SWITCHES

either of two switches; one on the oscillograph control-panel and one on the combined data- and control-board (Fig. 1 C). Light tight film-magazines with a 200-ft. capacity are employed for both the exposed and unexposed recording paper in order to make daylight loading possible.

Detector-equipment. Small rubber tubing (3/32 x 3/8 in.) stretched halfway across the pavement is anchored in place by means of stakes, small turnbuckles, and soft iron stovepipe wire threaded through its center. These rubber tubes are quite inconspicuous to the driver, since they look like expansion-joints, and are used to actuate tambour pressure-switches (Fig. 2) at the side of the road. The switches are connected in the circuit (Fig. 3) in such a manner that pressure-impulses resulting from the passing of a car over the rubber tubes operate the switches which in turn shunt out the 15,000-Ω resistor in the deflecting plate-circuit of the cathode ray tube, the result being a momentary increase in the amplitude of the curve on the tube-screen. With the recording paper in motion, the result of this action is an oscillogram quite similar to the one in Fig. 4 which was made with the optical lever-recorder.

Operation. Two rubber tubes are stretched across the pavement at a known

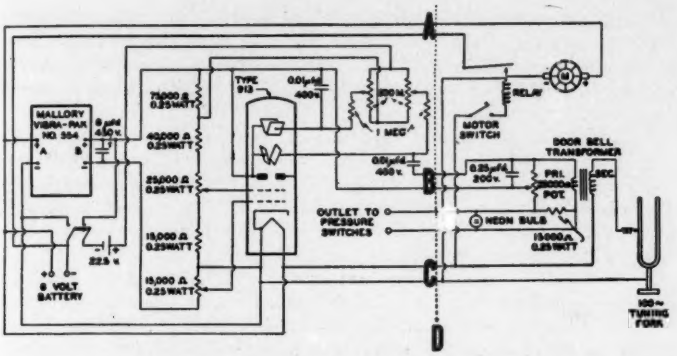

FIG. 3. CIRCUIT-DIAGRAM OF THE CATHODE RAY RECORDER

For the optical lever-recorder, only the portion of the circuit to the right of the broken line 'D' is used. The two pairs of leads 'A' and 'C' are connected directly to the 6-v battery and the pair marked 'B' is attached to the headphone unit.

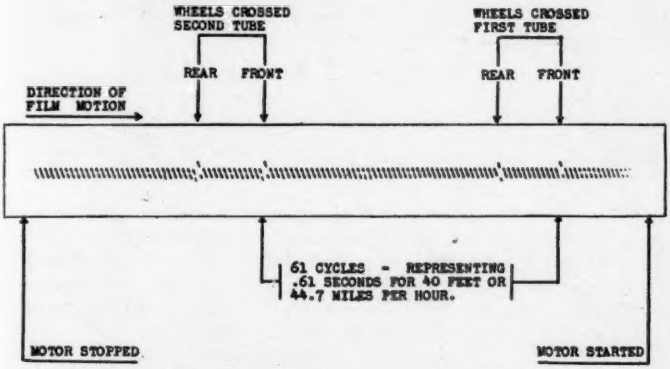

FIG. 4. AN OSCILLOGRAM MADE WITH THE OPTICAL LEVER-RECORDER

Oscillograms made with the cathode ray recorder are quite similar.

distance apart, preferably 40 ft. (the minimum separation that will accommodate the maximum legal length for vehicles in the state of Indiana). Pressure-switches are attached to these tubes and cables are laid to some inconspicuous point sufficiently far from the highway so as not to distract the motorist. The cables are led to the control-board where a neon lamp is shunted into the circuit. This lamp glows continually as long as the motor-mechanism is off. When a car approaches the detector-equipment, the investigator turns on the motor-switch which extinguishes the neon lamp and starts the oscillographic record. As the front and back wheels pass over the

rubber tubes the pressure-switches are closed and deflections are made in the oscillogram. The neon lamp flashes on every time the pressure-switches are closed, hence, the investigator can readily see whether the switch-points are contacting. After a car has passed the detector-equipment, the apparatus is turned off and the investigator awaits the approach of the next car.

Reading the records. Records are read, after the paper has been developed, by counting the cycles between the deflections resulting from the passing of the front wheels over the first and second rubber tubes. Since a 100 ~ tuning fork is used, the number of cycles intervening shows in 0.01 sec. the time required for traversing the distance between the tubes. Conversion to miles per hour is accomplished by means of tables prepared for the purpose.

Calibration. Two methods have been employed in calibrating the instrument. The first was a laboratory test designed to determine the effects of varying amounts of clearance between the contact-points of the pressure-switches. A 16-lb. shot was held at the top of an inclined trough by a release-mechanism. Two rubber tubes were placed across the trough in such a manner that they would be traversed when the shot was released. The contact-points of the pressure-switch of the second tube traversed were set at 0.01 in.; those of the first tube were set at 0.005 in. in one

TABLE I

COMPARISON OF MEASUREMENTS WITH MINIMUM AND MAXIMUM CLEARANCE BETWEEN SWITCH-POINTS

	Clearance	
	0.005 in.	0.035 in.
No. measurements	25	24
Mean time (in 0.01 sec.)	89.04	89.27
S.D.	1.34	0.56

series of experiments, and at 0.035 in. in a second series. (These values are the extremes at which the pressure-switches operate successfully.) As shown in Table I, there is virtually no difference between the arithmetic means of the readings at the two settings. The variability of the readings was, however, considerably greater when the points were farthest apart. It is, therefore, considered advisable to operate the switches with a minimum clearance between the contact-points.

The second check was made in the field with the aid of a stopwatch. A section of smooth, level highway was chosen and a quarter-mile stretch was marked off by means of white stakes at the side of the road. The speed-recorder was set up in the center of this quarter-mile. A car was driven over this strip at as constant a speed as possible, and an investigator riding in the car clocked the time between the two stakes with the aid of sights. Records of the runs were taken simultaneously with the speed-recorder. While it is probable that this instrument is more accurate than timing with a stopwatch, a comparison of the speeds obtained by the two methods (Table II) shows an average difference for eight trials of 0.36 mile per hour.

THE OPTICAL LEVER-RECORDER

The optical lever-recorder is identical to the cathode ray recorder except that the cathode ray unit was removed from the instrument case (Fig. 1) and a mechanical

unit (Fig. 5) was substituted. This assembly consists of a lamp housing containing a 6-v. 50 candle-power bulb, a Dorsey phonelescope[2] (incorporating the optical lever-principle) actuated by a standard headphone unit, and an appropriate lens system for focusing the light beam on the recording-paper. The circuit used is identical to that portion of the circuit-diagram at the right of the dotted line 'D' in Fig 3. The two pairs of leads marked 'A' and 'C' go directly to the 6-v. battery and the pair marked 'B' are attached to the headphone unit. The 6-v lamp is connected in the motor-circuit so that it operates only when the motor is in motion, thus preventing unnecessary heating of the lamp. Fig. 4 is an unretouched photograph of an oscillogram made with this unit. All other features of the equipment and its operation correspond to those of the cathode ray instrument.

The Multiple Recorder

The multiple speed-recorder is an elaboration of the apparatus already described and may utilize either the electronic or mechanical principles discussed above. It consists of the basic recording unit with the addition of more rubber tubes and

TABLE II

Comparison of Speeds Obtained with a Stopwatch and with the Cathode Ray Recorder

Trial	Miles per hour		Difference
	Watch	Instrument	
1	30.4	30.1	−0.3
2	30.4	30.7	0.3
3	30.4	30.7	0.3
4	41.3	40.9	−0.4
5	41.7	40.0	−1.7
6	49.2	49.8	0.6
7	50.3	50.5	0.9
8	50.0	50.5	0.5
Av. diff.			0.56

pressure-switches. Six of these spaced at 100-ft. intervals have been used and the performance of the instrument indicates that many more could be added with equal success. One common wire is connected to all switches and a separate lead extends from each one to a selector-switch on the control-board.

Operation. As a car approaches the wired area, the motor of the apparatus is started and the selector-switch is set so as to connect the instrument with the first pressure-switch. After the vehicle has crossed the first tube (as is indicated when the neon lamp flashes) the selector-switch is set for the second rubber tube, and so on. In this manner it is possible to follow the same driver through the entire 600 ft. and his average speed for each consecutive 100 ft. may be obtained. Since only one pressure-switch is connected with the oscillograph at a time, very few records need to be discarded because of interference from passing or trailing cars.

Use. Use of the multiple speed-recorder has been confined to date to the study of the deceleration-patterns of drivers in response to various types, sizes, and locations of highway signs. Fig. 6 shows a graph of the patterns of five drivers approaching

[2] C. H. Stoelting Co., 424 North Homan Avenue, Chicago, Ill.

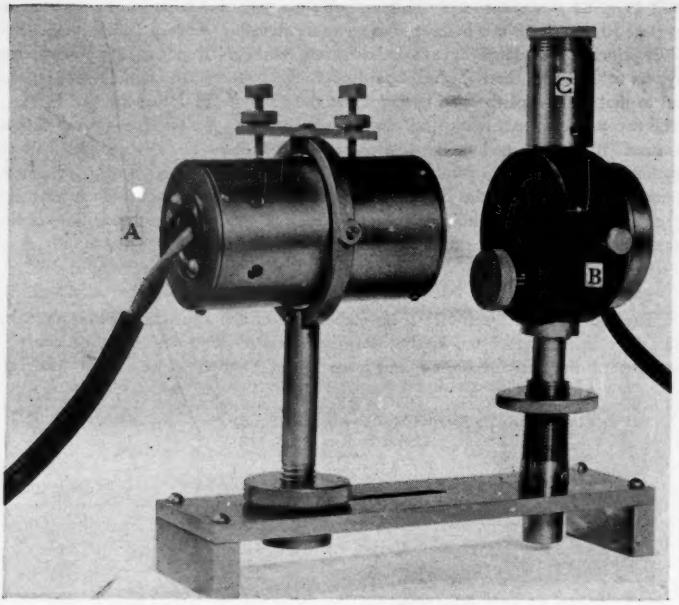

FIG. 5. THE OPTICAL LEVER-RECORDER
'A' is the 6-v. lamp-housing and 'B,' the phonelescope with headphone unit attached. 'C' is the lens system for focusing the light beam on the recording paper.

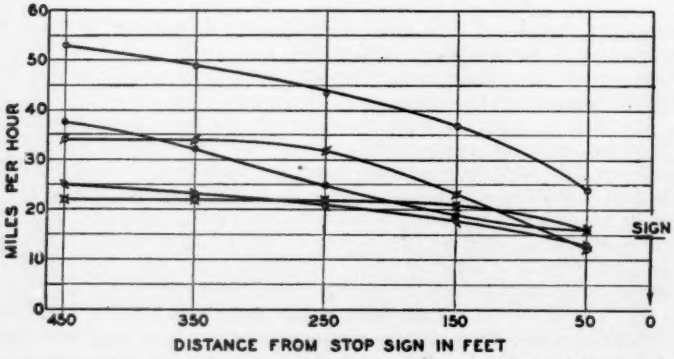

FIG. 6. DECELERATION-PATTERNS OF FIVE DRIVERS
All of the drivers were approaching a standard sized stop-sign (24 in.) at a state road intersection. These results were obtained with the cathode ray recorder.

a standard 24-in. stop-sign at a state road intersection. It is representative of the type of data that may be collected with the instrument. The apparatus may also be used in a number of situations such as those involving parked vehicles, pedestrian crossings, and advertising signs, where a continuous speed-pattern of drivers is desired in studying their behavior on the highway.

Comparison of the Devices

The electronic and mechanical recording devices described here have both been successfully used. It may be desirable, however, to point out, from our experience with them, the advantages and disadvantages of each.

The chief advantage of the cathode ray recorder is that its first cost is less. After extended use, however, the cathode ray tube tends to lose its photographic properties. There is also some evidence that at times the paper passing across the glass screen develops an electro-static potential which disturbs the action of the tube and results in a blurred record. The optical lever-recorder never fails to yield a readable record provided the instrument is properly focused. While records made with both instruments are adequate, those made with the optical lever-recorder are more clear-cut and easier to read. Records made with the optical lever-recorder are, moreover, much finer, hence, it is possible to operate the film at a slower speed—thus reducing operating costs.

A PROJECTOR-STAND TO FACILITATE PLOTTING

By L. C. Gilbert, University of California

To facilitate the labor of plotting eye-movements from motion-picture film an adjustable projector-stand has been designed, permitting the laboratory technician to sit comfortably at a table and work in a normal writing position. The stand rests upon a substantial work-table and consists of stable base (B) supporting on uprights a platform (C) which moves in grooves. The projector (P) rests upon the platform which can be lowered or raised by crank (A) to adjust the magnification to the reading distance of the eyes from the reading material during the photography. Directly in front of the projector lens the platform supports a mirror (M) at an angle of 45° in such a manner as to reflect the image downward to the movable plotting board (K) which rests flat on the table. For accurate reflection it is important that the mirror be of superior quality. The mirror stand (E) may be moved back and forth to adjust the distance between mirror and lens.

The film in the projector is manipulated from the table surface by the non-plotting hand by means of a chain and gear (G) attached to the projector sprocket (R). Plotted film passes into a container through a slot (S) in the platform base. For additional convenience there are three drawers built into the back of the base with pegs for the rolls of film and two drawers without pegs for plotting paper and pencils.

The movable plotting board rests flat on the table. The record from the elevated projector is reflected down to the plotting paper. The technician can then sit comfortably at the table and record the data in a normal working position, without the

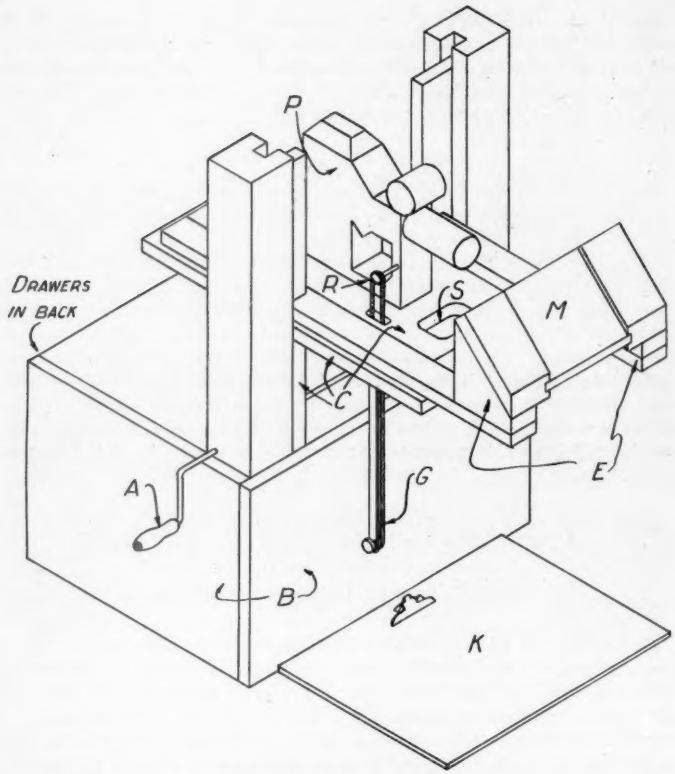

FIG. 1. PROJECTOR-STAND
A = crank; B = base; P = projector; M = mirror; E = mirror stand; S = film slot; R = projector sprocket; C = projector platform; G = chain and gear; K = plotting board.

necessity of shifting back and forth to turn the film and without the discomfort of plotting against a vertical board.

A SIMPLE AND ACCURATE METHOD OF CALIBRATING CHRONOSCOPES

By ROY M. DORCUS and GEORGE E. MOUNT, University of California at Los Angeles

One of the major problems in reaction-time work and in precise measurement of short time-intervals has been the calibration of the instruments. The old Hipp chronoscope was calibrated by means of the Wundt Fall Hammer or similar gravita-

tional devices. It is well known that the friction of the bearing supports of the pendulum, the inertia of the contacts as well as the friction in the contacts resulted in some appreciable error. Substitutes of falling balls or balls rolling down inclined planes are not completely satisfactory, since the contacts that are made and broken by falling bodies give trouble especially for the shorter time-intervals. Friction of rolling balls on an inclined plane is difficult to figure and hence time measures from this source are likely to be inaccurate. The other method that has been used and which is highly accurate is photography. This involves very expensive equipment, including a string galvanometer or oscillograph, a high speed camera, and sensitive film. If the circuit to be timed is photographed simultaneously with an oscillating circuit, the degree of accuracy will be as reliable as the accuracy of the oscillating circuit.

The method proposed here is a modification of the photographic method. Many laboratories possess apparatus for making speech-records and if they do not, records

FIG. 1. SAMPLE RECORDS
A = 512 ~ tuning fork; B = 1000 ~ oscillator

can be made commercially for a small cost by almost any radio sales store. All the equipment that is needed in addition to the record cutter is a tuning fork or an oscillator of known frequency.

The method of making the calibration with a tuning fork is as follows. (1) Set the recording equipment into motion. (2) Strike a fork and hold close to the microphone. (3) Close the amplifying circuit to the cutting head simultaneously with the starting circuit of the chronoscope. This can be accomplished with a single-throw double-pole switch. (4) Open the amplifying circuit simultaneously with the break circuit of the chronoscope by means of a double-pole single-throw switch. (5) Read the time on the chronoscope. (6) Count the oscillations cut on the record.

The procedure with an oscillator is the same as with a tuning fork except that the output of the oscillator may be fed directly into the cutting head.

It might be added that using a tuning fork is probably more reliable than using an oscillator since oscillators do not hold their calibrations accurately over long periods of time and since it is almost impossible to set them at a frequency which is exactly that specified on the calibration dials.

It is well-known that the beat count technique is useful in calibrating oscillating

circuits at low frequencies when compared with a tuning fork but at the higher frequencies (over 10,000 ~) the accuracy of adjustment of the oscillators is such that it is almost impossible to set them at a point where the beats can be counted. These beats can be determined, however, if the fork and oscillator are recorded simultaneously on a disk. In making this record, the input current from the microphone should be equated with the current imposed on the cutting head from the oscillator. This is an inexpensive method of calibrating certain of the frequency-ranges of the oscillator.

We have presented in Fig. 1 photographs of a sample record of a fork and oscillator. The arrow in A marks the end of the vibration of a 512 ~ tuning fork; the arrow in B marks the beginning of a 1000 ~ oscillator. The rate at which the record is revolved will in part determine the ease of counting the oscillations. We have found that aluminum records are easier to read than acetate records. Since the cutting head responds to very high frequencies we did not expect any appreciable lag but this point was checked and no lag was detected at the 1000 ~ frequency.

A NEW DEVICE FOR MEASURING THE GALVANIC SKIN RESPONSE

By S. EDMUND STODDARD and CARL FISK, Blackfoot, Idaho

The device described here for measuring the galvanic skin response has been successfully used by the authors for three years. Its simplicity of construction and operation warrants, as we believe, its wider application and use.

The device, shown on its side with top removed in Fig. 1, consists of a light-proof box (A)—88 cm. long, 22 cm. wide, and 20 cm. high—which contains a 100-w. mazda lamp (B) and a photronic cell (C). The photronic cell is placed in the small box (F)—16 cm. long, 10 cm. wide, and 9 cm. high—, whose lid (L) is also removed, at any one of four positions. At the front of this small box an iris diaphragm (F) is mounted whose aperture ranges between 2.5–30 mm. in diameter. The mazda lamp (B), which is directly in front of the iris diaphragm, is fastened to a base that may be moved either forward or backward between the supports (E) by means of the metal rod (K). There are then three ways of varying the strength of the light falling upon the photronic cell: 1) the position of the cell within the smaller box; (2) the size of the iris diaphragm; and (3) the distance of the mazda lamp from the iris diaphragm. Of the two wires from the photronic cell, one runs to the galvanometer (D) and the other to one of a pair of copper electrodes (G). The second of these electrodes is connected to the galvanometer.

The placing of the electrodes on the skin is of great importance. We place them on opposite sides of the palm of one hand. Two pieces of felt are first dampened in a normal saline solution and placed in position. The electrodes are then laid upon the felt and held in place by means of a broad rubber band. The rubber band insures a constant pressure of the electrodes against the skin.

In this set-up we used a Weston photronic cell and a Leeds-Northrup galvanometer. The Weston cell is not injured by direct sunlight; it is non-microphonic and comes to its maximum value instantaneously—declining slightly after a few minutes

to its final value which is approximately 1% below the maximum. Temperature changes have little effect on the current output though they do have relatively

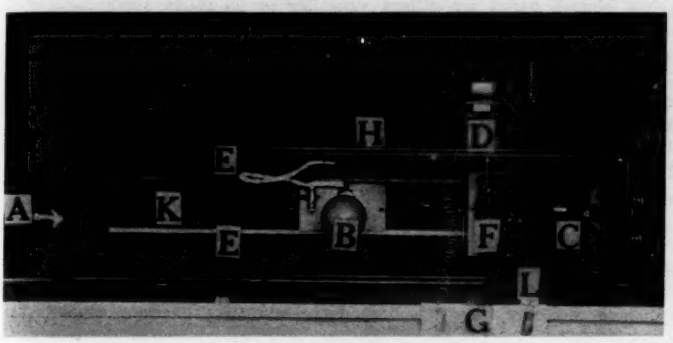

FIG. 1. THE PHOTRONIC APPARATUS FOR MEASURING THE GALVANIC SKIN RESPONSE

a large effect upon the voltage generated. The galvanometer has a sensitivity of 0.31μ a per mm. Its period is 2.7 sec. The external critical damping resistance is 2800 Ω, and the coil resistance is 1110 Ω.

The results that we have obtained with this apparatus have been highly satisfactory.

APPARATUS NOTES

THE USE OF MUNSELL PAPERS IN TESTS DIAGNOSTIC OF COLOR WEAKNESS, COLOR BLINDNESS, AND VOCATIONAL COLOR CAPACITY

To meet shortcomings in current clinical tests of color vision, test-patterns need to be extended in two directions: (1) into more saturated color regions, in order to isolate the completely red-green blind; and (2) into weaker chromas, to detect border-line cases of high or low sensitivity to single critical colors, *i.e.* red, blue, violet, green, or yellow.[1]

---

* Read at the annual discussions session of the Problems Committee of the Inter-Society Color Council, February 22, 1940, New York City. For apparatus and materials, the writer is indebted largely to grants-in-aid from the National Research Council and the National Society of the Sigma Xi; for other equipment and space, to the Psychological Laboratory of Cornell.

[1] Tests of foreign manufacture are not only difficult to obtain under war conditions, but impossible to copyright, hence subject to ruthless exploitation by commercial interests. Their use of easily memorized digits tempts applicants for whom normal color vision is a requisite to dishonesty (of this the writer has abundant proof). Cf. E. Murray, The Ishihara test for color-blindness, this JOURNAL, 47, 1935, 511-513. Use of numerals of unequal legibility, and the unhandy distance to which the plates are keyed for testing, invariably ignored by examiners, inject extraneous and variable vectors arising from refraction, astigmatism, acuity, retinal color zoning, and other ocular factors into test results.

Sample designs used in attacking this problem are of three types: *(1)* hue threshold; *(2)* hue discrimination; and *(3)* hue identification charts. In these designs Munsell papers are utilized, since they permit precise specification of the three dimensions employed in psychological color description, and are further standardized by spectrophotometric curves.[2]

(1) Hue threshold charts. A low-chroma hue (for red, blue, green, or violet sensitivity) is displayed through a circular aperture (1/16 to ¾ in. in diameter) in a matching gray ground. *E.g.* PB 5/2 through a ½ in. aperture in a 3½ in. square of N 5 (a middle gray). The chroma of the hue may be varied until *O*'s threshold at reading distance is roughly located for a given spot of color. Variation of distance is to be avoided, for reasons connected with refraction.

Gray backgrounds of the same value are used in preference to black or white as being less likely to involve hue shifts or changes in chroma in small color areas, though 'simultaneous contrast,' *i.e.* induction of negative hue or brightness by the ground. Exposure is made in a 3½ in. sq. gray kodak printing-frame with or without a protective layer of cellophane or celluloid. The frame permits ready

---

[2] Extended use of Munsell designations has been made by the ISCC committee on specification of colors in their recently completed task (begun in 1931) of standardizing color names for the U.S. Pharmacopoeia. The end set was a system of designations "sufficiently standardized to be acceptable to science, broad enough to be usable by science, art, and industry, commonplace enough to be understood by the general public." By utilizing the Munsell framework, 319 terms were found sufficient (with modifiers) for the entire psychological color solid, comprising millions of color tones. See D. B. Judd and K. L. Kelly, Method of designating colors, *J. Research N.B.S.*, 23, 1939, 355-386. This system is known as the ISCC-NBS method, in recognition of the joint contributions of the Inter-Society Color Council and the National Bureau of Standards.

The Munsell system was first brought out in 1905 and 1913, by Albert H. Munsell of the Massachusetts School of Art, after years of intensive work on Helmholtz' suggestion for tri-dimensional description. Since Munsell's death in 1918 the system has been further elaborated by A. E. O. Munsell and the Munsell Company. (See *Munsell Book of Color*, standard and abridged, with its 400 color specimens, and the *Munsell Manual of Color*, of 1929.) Equally spaced on a hue circuit of a 100 steps are five basic hues: red, yellow, green, blue, and purple. These, with the five intermediates, YR, GY, BG, PB, and RP, constitute the major hues, to each of which is allotted a 10-step span on the hue circuit, with the hue centered on step 5. Ten secondary intermediates, R YR, YR Y, etc., appear also on the published color charts (for simpler designations, *cf.* Judd and Kelly, *op. cit.*, 359, and D. Katzin and E. Murray, Instability in color naming, this JOURNAL, 46, 1934, 613). All 20 are available also in a number of values and chromas in separate sheets or smaller swatches, the number of variants in a constant hue chart ranging from 7 to 32 according to the permanent pigments obtainable.

Value (lightness) is graduated in 10 steps from black (zero) to white (10), on the central axis of the color solid. Values on the constant hue charts range from 2 to 8. Chroma (vividness or strength of color) is figured also on a 10-point scale. A given sample may therefore be designated by the symbols P 5/4, the numerator representing value, the denominator chroma or saturation. For greater precision a hue circuit digit (1-10) may be prefixed to the hue letter as 5R 6/10. Chromas 2, 4, 6, 8, 10, and sometimes 12 and 14, are now available for a number of the major hues; 4, 6, 8, or 10 for most of the secondary intermediates, and lately for some of the third intermediates, *e.g.* 2.5 and 7.5 R. (The Allcolor Company offers a number of supplementary chromas, though less accurately scaled, for the use of illustrators, advertisers, and interior decorators.) The neutral grays of the value axis are designated as N 1-10.

shift of the color behind the aperture, and helps eliminate the shadow halo, especially if a celluloid cover can be used without alteration of hue or annoying reflections. *Query* (put to the *O* tested): Is a spot visible on the ground? If so, is it the same or different in hue or value (brightness)?[3]

Reverse procedure displays a matching gray through an aperture in a square of low-chroma hue, thus ruling out reduction of color through *macula lutea* absorption (an end served also by enlarging the aperture).

(2) Hue discrimination charts: (a) One-spot. Low-chroma colors nearby on the hue circuit and of the same brightness are displayed one through an eyelet of another and vice versa. *E.g.* G 5/2 through BG 5/2. The difference between the two hues may be gradually increased or decreased till the approximate limen for discrimination of hue for a given individual in this region is located. [This method may also be used to test confusion-colors in the color-blind.]

(b) Two-spot. Two hues of low or medium chroma are displayed through 1¼ in. apertures (about 1 in. apart) in a 6 x 3½ in. rectangle of gray of the same value. *E.g.* G 5/4 and GBG 5/4 (green and bluish green) on N 5, in a larger kod. k frame (painted gray or black). As in (*a*), chroma, value, or hue difference may be varied, or other regions of the color scale substituted. *Query:* Are two spots visible on the ground? If so are they the same or different in hue or value from the ground? From each other? Which is bluer or greener?[4]

The above charts, *(1)* and *(2)*, are designed primarily to spot divergences in *sensory* (biological) *equipment*, innate or acquired, producing color blindness or color-weakness and imposing inevitable limits on sensory performance and color aptitude. The patterns of *(3a)* and *(3b)*, on the other hand, presumably probe not only intrinsic and inalterable differences in sensitivity at various points in the hue circuit, but chance differences as well in color concepts and speech habits, such as the location and extent of the hue range accorded a given term (green or blue) as a result of early environment and training; or the ability to differentiate between hue and chroma or brightness changes (artificial and acquired distinctions).

(3) Hue identification and recognition charts: (a) Three-spot. Choice of two alternatives (in the violet to purple region). A standard and two comparison hues, of the same or slightly different brightness and chroma, are displayed diagonally in the square kodak frame through ¾ in. circular apertures in a matching gray. *E.g.* P 5/6 between PRP 5/6 and RP 5/6 on N 6. *Query:* Which of the two comparison spots is the closer match (*i.e.* nearer on the hue circuit) to the central standard?

---

[3] Hue threshold tests, of the old Abney and Hess 'extinction of color' type, are indispensable prerequisites in determining color aptitude. Obviously, the limiting factor in an individual's discrimination of hues is his relative sensitivity to the several primaries, the overlapping of which gives us the various discriminable colors of the spectrum, to the number of about 150. Weaken one primary and the number of adjacent discriminable hues inevitably declines. Superimposed upon these biological optical limits (which may or may not be alterable by drugs or diet) will be the individual's learning curve in this field of interest.

[4] The stock psychophysical techniques (methods of limits and of constant stimuli) are applicable here. Where, however, interest pivots on color aptitude rather than on color theory, and where a sense organ as delicately adjusted as the eye is concerned, a dozen spaced and carefully conditioned observations will be worth far more than several hundred rapid-fire judgments (with an over-adapted *O*) rushed through to run up impressive statistical totals.

Which is bluer? [The blue-weak may hesitate between the two comparison colors. Trained color-blinds, on the other hand, guided merely by chroma differences in the blue component, may choose correctly; in which case the chroma of RP may be increased, that of the standard decreased, usually to the confusion of the red-green blind.]

This design may also be used with normal *Os* to test the accuracy of assignment of low-chroma hues on adjacent Munsell hue charts. *E.g.* many of the low-chroma Ys probably belong on the YGY page; the low-chroma Rs in the RPR group.

Similarly, in a higher zone of the value scale, tints of violet and purple may be compared for relative closeness of match. *E.g.* standard RPR 8/6 with PRP 8/2 and P 8/2 (lower chromas) on N 8 (a light gray). [Red weakness may lead to hesitation between the comparison violets. Blue weakness is less likely to lead to confusion.]

(b) Five-spot. Selection of match from four alternatives. The central standard in the sample is a bluish green, GBG 6/4. The four comparison colors are a second bluish green of lightly different chroma and value, GBG 5/6; a green, G 6/4; a greenish blue, BGB 6/6, and a blue-green BG 6/4. *Query:* Which of the four corner spots resembles most in hue the central standard spot? Response may be checked by four different positions of the test chart.

[Slightly divergent chromas and values are utilized in the five spots. Hence those of normal vision who have not learned to abstract hue from other variables may unwarily choose BG 6/4. A blue-weak individual may hesitate between the first two comparison colors, GBG and G.][8]

The sample procedures above described are all carried on under fixed or controllable conditions of illumination, surroundings, background, distance, size (visual angle), angle of stimulus-surface with line of vision and of lighting, and with constant pre-adaptation. A gray booth, Corning day-light roundels with high wattage bulbs yielding light of spectral energy distribution equivalent to 6500° K, illumination of 20 f.c., reading distance of 15 to 18 in., are among the constants.

All results needed for scientific norms should be checked with color vision thresholds obtained with spectrometer light or Wratten and Corning filters in a special limenopter with Aubert diaphragm or similar control of light intensity.

Preliminary tryouts included use of a Group Color Classification Test in which 36 five-inch samples of assorted hue, brightness, and chroma were displayed on a wall chart beneath 6 standards (*viz.* hues of middle to high chroma—Y, G, BG, B, P—along with a medium gray). Every sample was allocated to its proper hue by a check mark on a cross-section blank, designed for correction with a stencil. Color-blinds were found to be as effectively eliminated by this test as by the Holmgren wools or the Ishihara. Its use for more exact diagnosis, however, was blocked by difficulty in securing accurate hue, chroma, and value sequences for the six standards in the Hering, Heidelberg, or Bradley colored papers available. Recent extensions

[8] The severity of the test may be increased by insertion of a time factor, *i.e.* presenting standard and 4-spot comparison chart in sequence. Results in any region, by either method, obviously bear on the industrial problem of 'color tolerances,' *i.e.* the amount of chromatic deviation permitted the manufacturer by the customer.

of the Munsell series, along with price reductions, raise hopes of the revision and more extended usefulness of this test procedure in preliminary diagnoses.[6]

A second preliminary test turned on the ability to arrange 20 hues of low chroma and value (4/4) in an evenly stepped circle (*i.e.* the Twenty Hue Circle Test). In an abbreviated procedure, the individual tested passed merely on the perfection or distortion of a set arrangement in which, at several points, there was interchange of adjacent hues. Both procedures catch color-blinds (to whom the concept of a color circle is fairly meaningless). Owing, however, to the fact that lower chromas were not available for more than 10 of the Munsell hues, whereas far more than 20 are needed to provide sufficiently small hue steps, the test proved impracticable for isolating mild color weakness. Recent extension of the Munsell system may permit revival in more adequate form of this procedure.[7]

On the basis of the above tentative approaches, a clinical color test is in process of construction by the writer. Utilizing in the main the old and tried device of a design in terms of color spots of varying size, chroma, and hue on a white ground, it aims to eliminate reliance on test numerals of varying legibility and high memorability. Further, it aims to diagnose both complete and partial red-green blindness, shortening of the spectrum at the red or violet end, and other borderline deviations in the normal-visioned, the detection of which, with the growing use of color in art and industry, has assumed the proportions of a major problem.[8]

Cornell University ELSIE MURRAY

---

[6] This test was administered in 1935 to a high school art class of 55, with a view to evolving a measure of amount of improvement in identification of hues induced by a semester's instruction. Initial difficulties, arising from the foreign parentage of many of the students, and their awkwardness in handling English prepositions, whereas the directions called for a check mark *opposite* the number representing the sample, and *under* the letter representing the standard, threatened to transform the exercise into an intelligence test, but were overcome. More than half of the class were girls, and color-normal; but among the boys two color-blinds and two color-weaks were discovered.

[7] The Student Charts developed by the late Walter J. Spry of the Munsell Company, published in 1936 and now in use in a number of colleges and art schools, designed to establish the three-dimensional concept of color in the student's mind, are built on similar lines. Diagrams of a 10-segment hue-circuit, a 9-step value axis, a 7-step chroma radius for R (covering 2 to 14 points), and an envelope of color chips (each about ½ by ⅝ in.) are provided at moderate cost. Ten or more constant-hue charts, each with its envelope of chips, and a silhouette indicating the characteristic outline of each completed chart, are now available. By mixing the contents of the envelopes for two or more adjacent hues, ability to discriminate hue from value or chroma might also be tested, especially when the present minor deviations from accuracy of the Munsell constant-hue charts are corrected. Timing of arrangements before and after a course of instruction, with due allowance for diverse finger agility, may gauge susceptibility to improvement in hue discrimination.

[8] While the tests above described are utilized by the writer mainly to locate critical color regions to be tapped in tests for color deviates, they are susceptible of other uses, *e.g.* in Psychology I laboratory exercises, where they may serve two purposes. First, they provide excellent material illustrative of the type scientific experiment, with exact control of conditions and quantitative variables. Secondly, they tap the natural undergraduate interest in individual differences and achievement, and suggest natural vocational trends or limitations—in an era when color has insinuated itself into a wide variety of occupations and professions. In the graduate laboratory, they assist in the selection of qualified *O*s for visual problems.

NOTES AND DISCUSSION

The Size of the Differential Limen for Pitch

The size of the differential limen for pitch has always been of especial interest because it is supposed to fix the minimal differentiation in the inner ear that a place theory of pitch, like Helmholtz's, must have. Helmholtz himself had only Seebeck's statement that the threshold is about 1/1000, and he figured that there ought thus to be about 64 discriminations per semitone, although there were only about 33 rods of Corti per semitone to act as resonators.[1] After Luft's determinations had come out in 1888, Külpe used them to conclude that there would be altogether about 11,000 discriminations in the total range of pitch. By this time Helmholtz had abandoned the rods of Corti in favor of the basilar membrane as a resonator, so Külpe merely remarked that there were more than enough hair cells in the inner ear to provide the necessary differentiation.[2] This figure—11,000 discriminably different pitches—is probably fixed in the mind of nearly every psychologist who took his Ph.D. before 1920; yet here are Shower and Biddulph, with the new thermionic technique for tonal stimulation, finding limens so large that the number of discriminable pitches, even at the intensity which gives the maximum, is only about 1500.[3] What has become of the other 9,500 pitches that Külpe had? The modern thermionic psychologists say that the old results were got with bad technique and without good experimental control, but why does *poor* control help the organism to make *more accurate* discriminations?

The conventional values that are usually cited for the chief investigations of the differential limen for pitch are given in Table I. Inspection of the table makes it appear that the limens, consistently small in the early days, got suddenly larger about 1900. Before we inquire into the reason, let us see what these investigations were like.

The early estimates of differential sensitivity generally stated the frequency ratio that could be just discriminated, and these fractions varied from 1/2000 (Sauveur, 1700) to 1/100 (Seebeck, 1846). They did not take into account the fact that the fraction might vary with the frequency.

The first adequately described experiment was made by Delezenne in 1827.[4] He had a string 1147 mm. long, tuned to 120 c.p.s. A bridge placed at the middle gave him two halves, each tuned to 240 c.p.s. He found that good *Os* under the

---

[1] H. Helmholtz, *Die Lehre von den Tonempfindungen*, 1865, 219 (undoubtedly the same in 1st ed., 1863, which I have not seen). Helmholtz attributes the Weber fraction of 1/1000 to E. H. Weber himself, but Weber seems to have been referring to Seebeck's determination.

[2] O. Külpe, *Grundriss der Psychologie*, 1893, 108-111, 114; or Eng. trans., 104-107, 110 f.

[3] Cf. S. S. Stevens and H. Davis, *Hearing*, 1938, 152 f.

[4] C. E. J. Delezenne, Sur les valeurs numérique des notes de la gamme, *Recueil des travaux de la société des sciences, de l'agriculture et des arts de Lille*, 1827, 1-56, esp. 4-6. This frequency, usually cited as 60 or 120 c.p.s., is really 240 c.p.s., as E. B. Titchener, *Experimental Psychology*, II, ii, 1905, 235-237, makes clear; but Titchener, who did not see the original article of Delezenne, is wrong about thinking that the threshold should be 0.83 instead of 0.42.

best conditions could detect a difference in pitch if the bridge were moved 1 mm. toward one end. That means a limen of 0.42 c.p.s. Practically everyone could notice the difference of 0.84 c.p.s. when the bridge was moved 2 mm.

Preyer used an elaborate set of reeds made by Appunn.[5] He confirmed Delezenne and Seebeck, and fixed the values for the twice and the thrice accented octaves. Thus far there was no use of psychophysical method: the limen was not a statistical quantity but the least difference observed under optimal conditions.

Luft used tuning forks with sliders to alter their pitch, and employed the method of limits.[6] Max Meyer decried the method of limits for this purpose, and with

TABLE I

DIFFERENTIAL LIMENS FOR PITCH

These figures, which are all expressed in c.p.s., are the conventional values usually given for the investigators named. The conditions under which they were observed are discussed in the text.

Frequency	Delezenne 1827	Preyer 1876	Luft 1888	Meyer 1898	Stücker 1908	Vance 1914	Shower and Biddulph 1931
31, 32			.44				1.26
62–73			.15		.70	3.40	2.64
100–131			.16	.54	1.00	1.40	3.10
200–262	.42		.23	.25	1.30	1.40	2.64
400, 440				.28	1.40		
500–512		.30	.25			1.80	2.60
600				.24			
870					2.50		
1000–1200		.50	.22	.69		3.30	3.60
1740–2048			.36		7.70	5.70	3.80
3100, 4000					26.70		9.20
6200, 8000					304.00		23.20
11700							41.00

tuning forks determined the limens for Stumpf's ear by the method of right and wrong cases.[7] Actually he gave only the frequency of right cases; the limens in the table were calculated for him by Vance. These two studies introduce psychophysical method but are defective in that they rely on the manual actuation of the tuning forks and fail to control the intensity of the blow.

Stücker used forks up to 522 c.p.s., strings for 870 and 1740 c.p.s., and the Galton whistle above 3100 c.p.s.[8] Instead of trying to test maximal sensitivity, he interested himself in individual differences, with especial interest in the musical training of his *Os*. His results in the table are averages. There is no evidence that he used a psychophysical technique.

---

[5] W. Preyer, *Die Grenzen der Tonwahrnehmung*, 1876, 26-37.

[6] E. Luft, Über die Unterschiedsempfindlichkeit für Tonhöhen, *Philos. Stud.*, 4, 1888, 511-540, esp. 528.

[7] M. Meyer, Über die Unterschiedsempfindlichkeit für Tonhöhen, *Zsch. f. Psychol.*, 16, 1898, 352-372, esp. 352-361.

[8] N. Stücker, Über die Unterschiedsempfindlichkeit für Tonhöhen in verschiedenen Tonregionen, *Zsch. f. Sinnesphysiol.*, 42, 1908, 392-408.

Vance was also concerned with individual differences and gives the data for 50 Iowa students.[9] He employed tuning forks, carefully and lightly struck, and he found his limens by the method of right and wrong cases with the *equal* category excluded Table I gives his averages of the 50 *Os*.

Shower and Biddulph provide the classical results for the new technique with thermionic oscillators.[10] They objected to the hand-struck tuning fork because this method of actuation introduces overtones and transients that can become, instead of the fundamental, the basis of the judgment. Any abrupt interruption or initiation

TABLE II

EFFECT OF PRACTICE ON THE DIFFERENTIAL THRESHOLD FOR PITCH

Averages of Stücker's thresholds in c.p.s. for four groups with different training. N, number of *Os*, is small because only those *Os* are included who had limens for all 8 of the frequencies used in the table; otherwise the averages would not have been comparable.

Frequency c.p.s.	Colleagues N=6	Trained musicians N=8	Singers N=6	Unmusical *Os* N=4
73	.99	.25	.18	1.26
130	.90	.39	.32	1.40
261	.91	.43	.46	1.92
435	1.74	.57	.68	3.05
870	3.22	1.14	1.23	6.15
1740	10.61	2.76	3.07	15.25
3100	35.34	8.75	9.50	54.00
4176	127.37	29.25	38.50	109.00
Av.	23.13	5.44	6.77	24.00

or change of a frequency necessarily introduces confusing transients. Shower and Biddulph chose, therefore, to vary the frequency gradually, changing the properties of the circuit according to a smooth sinusoidal function of frequency-change, mechanically controlled. They found that they got the smallest limens by varying the frequency at the rate of two changes per second, and they used this rate. They always began with equality and increased the difference until it was just noticeable.

So much for Table I. Now let us see what factors besides the fundamental frequency of the stimulus affect the size of the differential limen.

(1) Practice reduces the limen. From the very first the evidence has been that discrimination is best for the trained *O*, but Stücker's results make the fact sure. Table II shows how the limens are least for a group of trained musicians, larger for a group of singers, and largest of all for his unmusical observers. Thus it seems probable that Sauveur, Delezenne, Seebeck, Preyer, Luft (who used the members of his seminar in psychophysics as *Os*) and Meyer (whose *O* was Stumpf) ought to have gotten smaller limens than the averages of the less highly selected groups of Stücker and Vance, as indeed they did.

(2) Individual differences make the limen vary. Not all individual differences can be due to differences in practice and experience. The ear itself sometimes

[9] T. F. Vance, Variation in pitch discrimination within the tonal range, *Psychol. Monog.*, 16, 1914, (no. 69), 115-149, esp. 126 f.

[10] E. G. Shower and R. Biddulph, Differential pitch sensitivity of the ear, *J. Acoust. Soc. Amer.*, 3, 1931, 275-287.

limits discrimination. Moreover, the mechanism of judgment, as it varies even under identical instructions, can affect the limen. Table III shows that difference between the largest and the smallest limen in each of Stücker's groups is much greater than the differences between the averages of the groups.

(3) The method of limits and the method of right and wrong cases do not yield comparable limens. The method of limits, as Luft used it, measured the average spread of the *equal* category. The method of right and wrong cases, as Meyer and

TABLE III

INDIVIDUAL DIFFERENCES IN DIFFERENTIAL THRESHOLDS FOR PITCH

Averages, maxima and minima of Stücker's thresholds in c.p.s. at 435 c.p.s. for four groups; cf. Table II. N, the number of *O*s, is larger than in Table II because all the cases for 435 c.p.s. can be used.

	Colleagues N=7	Trained musicians N=16	Singers N=19	Unmusical *O*s N=14
Av.	1.82	.56	1.93	4.79
Maximum	4.22	1.10	9.80	8.70
Minimum	.04	.10	.15	.30

Vance used it, eliminated the *equal* category, and took the semi-interquartile range of the psychometric function as the limen. Luft's limens are thus partly dependent upon the attitude of the *O* with respect to the meaning of the categories of judgment; Meyer's and Vance's are partly dependent upon the variability of the organism's adjustment to the stimulus. These are such different determinants that the correlation of limens got by the two methods ought not to be perfect and might not even be high. See also *(5)* and *(6)* below.

(4) The limens of continuous change are larger than the limens for abrupt change.[11] It has even been suggested that they should be proportional to the rate of

TABLE IV

EFFECT OF CONTINUOUS AND ABRUPT CHANGE UPON THE DIFFERENTIAL THRESHOLD FOR PITCH

Shower and Biddulph's comparison of the thresholds at 40 db for continuous (sinusoidal) change and abrupt change. All figures are c.p.s. They have been read with a lens from a printed graph and are not (for continuous change) the same observations as those given in Table I.

Change	Frequency							
	63	125	250	500	1000	2000	4000	8000
Continuous	3.01	3.22	2.50	2.90	4.20	6.00	11.20	20.00
Abrupt	.97	1.56	1.87	2.10	3.00	4.00	10.00	14.40

change but there are no good data. This factor is derived from the relation of the judgmental process to the differentiation of the sensory field. It is easier to perceive a difference when the impressions are separated by a 'contour' than when the one fades slowly, spatially or temporally, into the other. Shower and Biddulph compared gradual with abrupt change, and it seems probable that the larger limens

[11] Cf. L. W. Stern, *Psychologie der Veränderungsauffassung*, 1898, esp. 208-243.

for continuous change at 2000 c.p.s. and above are due to this factor. See Table IV. Below 2000 c.p.s. much of the differences is presumably due, as they think, to transients and overtones. See *(7)* below.

(5) Fixed inequality between the standard and comparison stimuli favors smaller limens with the method of limits than with the method of right and wrong cases. Difference in the manufacture of forks or reeds may make it so that the nearest equivalence between the two stimuli is not nearly identity. The pattern of overtones or of transients may be different for the two instruments. If the *O* is to report *equal* in the method of limits, he has to make that judgment in the face of a difference. If the difference is clearly perceived, he abstracts from it only with difficulty. If it is not clear, it may nevertheless be effective. The result in the method of limits will be that *O* notes difference later in a series approaching equality, and earlier in a series that recedes from equality—thus making the limen less. In the method of right and wrong cases, as Meyer and Vance used it, the equality

TABLE V

LEAST VALUES OF THE DIFFERENTIAL LIMEN FOR PITCH

The results of the best Os are averaged for Stücker and Vance. The data for abrupt change of frequency are given for Shower and Biddulph, as well as the other investigators. All values are in c.p.s.

Frequency	Luft Av.	Stücker Av. 2 best musical Os	Vance Av. 6 best Os	Shower and Biddulph Abrupt change
62–73	.15	.08	2.45	.97
125– 131	.16	.13	.81	1.56
250– 262	.23	.21	.68	1.87
435– 512	.25	.25	1.37	2.10
870–1024	.22	.50	1.98	3.00
1740–2048	.36	1.15	3.53	4.00

judgments are excluded and the question is not, as in the method of limits, as to whether there is still a difference, but as to which way the difference lies. Extraneous differences may then confuse the judgment but they will not aid it, as they do in the method of limits. This factor explains part of the discrepancy between the thresholds of Luft and of Vance but not the similarity between the determination of Luft and Meyer.

(6) Variable inequality between the standard and comparison stimuli also favors smaller limens with the method of limits than with the method of right and wrong cases. The striking of tuning forks by hand is subject to great variation, and the strike transients create an adventitious variability which favors smaller limens with the method of limits. The argument is that same as that of the preceding paragraph.

(7) The presence of overtones and transients tends to reduce the limen for those frequencies which are below the region of maximal sensitivity (ca. 2000 c.p.s.). It is obvious that extra frequencies, varying directly with the fundamental frequency and lying above it, would tend to become the basis of judgment when they lie within a more sensitive region. It is also possible that lower transients might work similarly for fundamental frequencies above 2000 c.p.s. Table IV, showing all through its range the influence of continuous change, shows also the great effect of transients and overtones, an effect diminishing from 62 to 2000 c.p.s.

(8) The weaker the tone the larger is the limen. Shower and Biddulph's work

shows this relationship conclusively, but the effect is not great. The limen is nearly doubled only if the intensity is decreased from 40 to 10 db. This factor may enter into the difference between Vance's and Luft's results, since Vance seems to have tried to strike his forks gently, but it can not account for the main difference.

If we wish now in the light of our knowledge of these artifacts to lessen the discrepancies of Table I, we can largely eliminate the effects of practice and individual differences by taking the best *O*s for Stücker and Vance, as in Table V. We see at once that Luft's *O*s are no better than the best musical *O*s that Stücker had, so we may conclude that Stücker confirms Luft for thresholds got with hand-struck tuning forks. The limens for Vance's best *O*s remain higher, but that may be due to the fact that he used the method of right and wrong cases with hand-struck forks, instead of the method of limits, and furthermore that the two methods do not necessarily yield comparable limens. Since it is the procedure of the method of limits that gives the artifact here, Vance's results should presumably be preferred to Luft's. When we now compare Vance's best limens, got by abrupt change, with the limens that Shower and Biddulph got by abrupt change (Table V), we no longer find any great discrepancy. Vance, on the whole, finds smaller limens than Shower and Biddulph, a difference which may indicate that Vance had more transients than Shower and Biddulph.

There seems to be at the present time no way of determining the maximal sensitivity of the ear to difference of frequency without artifacts. For maximal sensitivity and comparable results, one should choose the most practised and most sensitive *O*s. The tones should be generated with thermionic oscillators and intensity should be controlled. If transients are not excluded the method of constant stimuli is preferable to the method of limits. In any case the method of limits should balance series in one direction by other series in the reverse direction, as Shower and Biddulph did not. The fundamental dilemma lies in the need to get rid of the transients that come from abrupt stimulation while still avoiding gradual transition from one stimulus to the other. The ideal is abrupt change without transients. Near 2000 c.p.s. abrupt change is presumably safe because the transients do not furnish good cues there. At low frequencies (*e.g.* 256 and 435 c.p.s.) maximal sensitivity probably lies somewhere between the indications given by abrupt change and by gradual transition. The 1500 discriminable frequencies given by the results of Shower and Biddulph are thus probably too few to express maximal sensitivity, but the 11,000 tones from Luft's data must be too many—very much too many.

Harvard University EDWIN G. BORING

MORE ABOUT THE 'BABOON BOY' OF SOUTH AFRICA

The note by Dr. John P. Foley in the January number of this JOURNAL (*supra* 128-133) was published prematurely before Dr. Raymond A. Dart and others in South Africa had completed their investigation of Lucas. The evidence now available appears clearly to disprove him as a feral case.

The recent resurgence of interest in the Lucas case is due to the writer's interest in feral material. In responding to a question relating to the 'Wolf children' of India.[1] Dr. Arnold Gasell of Yale University sent me a newspaper clipping re-

[1] Cf. P. C. Squires, "Wolf children" of India, this JOURNAL, 38, 1927, 313-315; W. N. Kellogg, More about the "wolf children" of India, *ibid.*, 43, 1931, 508-509; A further note of the "wolf children" of India, *ibid.*, 46, 1934, 149-150.

porting the case of Lucas. I relayed it to Dr. R. R. Gates of the University of Bristol, England, and he in turn sent it to Dr. Dart of the University of the Witwatersrand, Johannesburg, South Africa, who uncovered the police evidence which, though hearsay, made the case worthy of further investigation.

The early materials concerning the case of Lucas, accurately reported in Dr. Foley's note, were released to Dr. Foley and to others in keeping with Dr. Dart's generous policy of sharing his materials at all stages of the investigation with interested scientists. There was no thought on my part of publishing the materials until the investigation was complete.

Dr. Dart enlisted, in his investigation of Lucas, the assistance of Dr. E. G. Dru Drury, 120 High Street, Grahamstown, South Africa, and Dr. J. A. van Heerden of the Mental Hospital at Grahamstown—the institution where Lucas was treated in 1904. In a letter, recently received from Dr. Dart, copies of the reports of these colleagues were enclosed. The letter and reports are reproduced below.

UNIVERSITY OF THE WITWATERSRAND
JOHANNESBURG
Medical School

12th March, 1940

Professor R. M. Zingg
Department of Anthropology
University of Denver
Denver, Colo.
U.S.A.

Dear Dr. Zingg:

I enclose herewith a copy of Dr. Dru Drury's report on Lucas, the so-called "Baboon Boy," and also a copy of Dr. van Heerden's report.

With all the information now in our hands, it seems incontestable that the story, however circumstantial it appeared at first, proves a fiction.

With kind regards,

Yours sincerely,

(Signed) Raymond A. Dart
Professor of Anatomy

GRAHAMSTOWN MENTAL HOSPITAL

30th January, 1940

Professor Dart
Professor of Anatomy
University of the Witwatersrand
Johannesburg

Dear Professor Dart:

In company with Dr. Dru Drury I visited and examined Lukas—the so-called 'Baboon Boy'—on the 29th November, 1939. This native, whose age is computed to be now about 48 years, shows facial and anatomical characteristics of a cross breed between a native and a coloured or bushman.

His expression is fatuous and silly and this is aggravated by being partially edentulous. His demeanour is childish. He is clumsy and ungainly in all muscular movements, and stands with his feet splayed out. He salivates freely and, accordingly, "splutters" when he talks.

His language is English and he appears to know the significance of few, if any, words in Afrikaans or Native. He has a poor vocabulary in English but he is able to express himself fairly well.

He is reminiscent and appears to have a compulsion to relate how he acquired

the scar on his cranium. He uses the expression "Koelie kicked me." The word 'Koel' is used in some parts of South Africa by the farming community in referring to an ostrich.

His intelligence is that of a low grade imbecile. While his coöperation can be readily gained for the moment, he is distractible and it was found impossible to complete any standardized intelligence tests on him. He is unable to count to 5. He shows no planning capacity and has a total disregard of form or contour as shown by his handling of the Sequin Board.

He has no knowledge of money values—he calls a penny, shilling, and sixpence all "Oom Paul." This may refer to a period following the Boer War when coins of the South African Republic had an enhanced value and were prized and sought after and it has to be assumed that he acquired this term and other expressions, which he repeats, from the children of his mistress.

I cannot accept the story that this native was reared by baboons.

Yours sincerely,

(Signed) J. A. van Heerden
Medical Superintendent

A/AS

SUMMARY OF DR. DRU DRURY'S INVESTIGATIONS OF LUCAS

The first sight of Luke, on 24th November, 1939, was unexpected by the general practitioner, if not by the alienist. As we drove up to the homestead on the farm Thorn Hill, which has been Luke's setting for 35 years, he was standing inside a fence which enclosed a garden.

His khaki shirt and grey flannel shorts were newly washed and so was Luke. He grinned, showing defective teeth, and raised his hand in a salute. The general effect of clean preparation recalled many formal visits of inspection to institutions where a good impression on the visitor is desirable.

It appeared that Luke had been ready and standing to attention for more than 2 hours. As we passed to the house, this misunderstanding and the need for discussion of finance led to further delay. The lady of the house instantly denied the identity of Luke with the Lucas photographed in the Mental Hospital in 1904 and loudly called on the rest of the party, who were dining in an adjacent room, to support her. When a working arrangement, if not cordiality, had been reached, Luke was still standing in the burning heat, and still smiling—the well trained orphan, though nearly 50 years of age.

Luke followed us to the gable end of the stable where field measurements, photographs, and plaster impressions of hand and foot were taken. Luke was unwilling to take off his clean two-piece suit and was sluggish in his movements. The orders were given him by a man he had known since 1904, who spoke in simple English. Similar muscular clumsiness was apparent at his visit to the X-ray Department at the Settlers Hospital on November 30th. Getting off and on to the table, or posing on all fours, Luke showed no agility of movement. On starting the examination, the orphanage boy effect gave place to that of the natural or village idiot boy. Given a white tennis shirt, Luke seemed pleased. Offered sweets, he did not fill a cheek pouch but put them in a pocket and ate a few deliberately and without greed.

His measurements do not differ materially from those of any half-breed. His forearms are very well developed and so are his back muscles. This may well be due to 30 years work as blacksmith's striker and bellows blower. It is related that he could keep up such work for the whole day and never asked leave to go and relieve himself. The results to his clothing were disastrous, unless he got an order to go to the bush.

His general build, prominent buttocks and long head suggest an admixture of Bushman or Hottentot blood, under his kafir skin.

For what it is worth one may quote a statement ascribed to Luke, and assented to by him in our hearing, that his mother ran away with a Hottentot named Kleinbooi. One cannot feel, however, that any statement of Luke's is worth credence. He did not volunteer any remark except that one of us was "a good

doctor." This was on the second meeting at which also sweets were provided. None of his answers to leading questions would have survived a cross-examination.

It will be apparent that the front point of the semi-circular scar, and the beginning of the almost vertical depressed furrow in the skull are within an inch of the region of Broca's convolution in the fore brain, in the motor area for speech.

Three months of preliminary research had discovered his official record, all too scantily recorded by the late Dr. W. L. A. Leslie, and a photograph taken by W. S. Colegate. It has provided interviews with Mrs. Ethelreda Lewis and Mr. W. Muscott, postmaster at Port Alfred W., who are principally responsible for telling South Africa, England, and America, since 1928, about the history of Luke, as told by his deceased employer; and it has traced, with the help of Mr. F. R. Paver, editor of *The Star,* the earliest known reference to Luke in print, which appeared in *The Star* of 16th July, 1927.

There was an album published for publicity purposes. It contained a photograph and a few words summarising the baboon story, both probably due to Mr. H. Miles, "a member of the staff of the Railway Department Publicity Office." This album and picture postcards of "Luke, the Baboon Boy" priced at 1/6 used to be sold by Mr. T. Brandt, who was proprietor of the Grand Hotel, Port Alfred, during some years, from about 1928 to 1932. A copy of this photograph was obtained from the present proprietor, Mr. W. W. MacFarlane; it was his last copy.

Investigation of the police evidence supplied by Lt.-Col. C. J. T. Horak, Dept. Commr. of S.A. Police, Eastern Division, showed that actually no record bearing on the case exists, so far as is known. In 1921, that is, 17 years after Luke's admission to the Mental Hospital, the late Lance-Sergt. Charles Holsen, on a single occasion, related the story of the baboon troop to Const. W. J. Coetzer. When the latter came to Kafir Drift station, 5 years later, he heard the local story and recalled Holsen's account, which he had heard now 18 years before. It must be hard for any man, however trained in evidence, to separate Holsen's words from those of George Smith, who habitually shoed the police horses.

The value of Luke's "statement" of 152 words of narrative to (M) Const. G. G. Wright on 8th May, 1939, at the instance of the Deputy Commissioner of Police must be heavily discounted, in view of the mental processes of Luke.

Evidence as to the existence of the baboon story in 1904 is hard to obtain. Dr. Leslie's notes make no reference to it. They trace Lucas to Burghersdorp Gaol as an indoor pauper; "nothing being known about him or his people." Dr. T. D. Greenlees, the medical superintendent (now deceased) was on leave at the time. [Krisch—confirmed by Dr. C. G. Cassidy.]

The age of this acutely maniacal Kafir herd boy is given on the hospital records as 13 years, but the photograph suggests a boy of 10 years or less. He is identified with Luke by the scalp wound and X-ray evidence of what in 1904 was a badly united fracture of the right tibia. He was certified at Burghersdorp by Dr. Herbert Caiger and the late Dr. J. Tandy Bolger on 19th and 20th February, 1904, respectively, and the condition ascribed to his head injury. He was emaciated, dirty and destructive in his habits. He used English speech then and in the Mental Hospital.

On 20th February, 1904, a Basuto shepherd, aged 45, was also certified by Drs. Caiger and Bolger.

On 30th March, both patients were admitted to the Grahamstown Mental Hospital. In the hope that such a dual event, especially if the Baboon story were then in existence, might be recalled by him, I wrote to Dr. Caiger, now of Sheffield, Yorks. He has no recollection of the occurrence. With his help I am trying to trace one of the gaolers (Proffitt, Pearce, and Winnacott) who might have held office at that time.

The period of escort from Burghersdorp to Grahamstown alone remains to be

traced. If that took place by train, Luke's history from February, 1904 up to date would be accounted for by documentary evidence.

The baboon story must then be shifted from Driefontein, Koonap, and placed in the neighbourhood of Burghersdorp, miles away, or it must be discarded as a baseless piece of folklore, as applied to Luke.

George Howell, 6th November, 1939, said: "I am 84 years old. I was in the native wards at Fort England in 1904. I knew Lucas when he was in the native wards. We did not hear anything about the baboon story when he was there. I reckon it was made up afterwards." [The latter was volunteered; not given in answer to questions.]

A. Hobley, now of Kroonstad, says that he cannot identify Lucas. "I remember the case but cannot remember under what name he was admitted." He had read the *Outspan* and other accounts "a few years ago." [The boy was admitted as "Lucas," although George Smith said that he had given him the name.]

Wallace Stephen Colegate, aged 73 years, who died a few weeks later, had photographed Luke in 1928 and 1930 for Mrs. Lewis and Mr. Muscott respectively. He, living at the Kowie, was accustomed to the story. He said: "I used to photograph cases as a routine [at Fort England]. There were then two blocks for native patients. Old Van der Merwe, the attendant, brought Lukas to me for a photograph and said 'This is the boy who grew up with baboons.' Lukas was in the hospital for some weeks [actually 10 weeks]. I am pretty sure he was certified [correct]. I very much doubt the truth of the story. No one at the Mental Hospital took it seriously." Colegate noted that leading questions put by Mr. Muscott, about raw food and insects, had on a later visit been incorporated in the legend. [Colegate's facts are correct. Native admission to the hospital began in 1875 and ended in 1908, when such cases were transferred to Fort Beaufort.]

George Edward Krisch was a mason at the Asylum in 1904 and struck Dr. Cassidy, who interviewed him at 2 Kyle Street, Main Road, Strand, Cape, as a fine type of man physically, intelligent and reliable.

Krisch stated correctly that Dr. Greenlees never saw the boy Luke and that Dr. Leslie took the notes, but he denied that he was certified. "The boy could walk erect, but frequently went on all fours and used to fossick among the bushes looking for crickets, which he ate. The staff learnt about the boy from the press and were all curious to see and observe him and soon concluded that the story about him was all mere nonsense. He spoke English and knew no native language. He seemed to Krisch to be a boy of 7 or 8 years of age." [Dr. C. G. Cassidy—interview.]

Krisch confirmed attendant Bruce's sudden death while on duty at Valkenberg Mental Hospital of heart failure, some time in 1925. This is confirmed by ex-head attendant J. Linckner, who assisted at his post-mortem. Dr. J. A. van Heerden confirmed this.

Mrs. Bruce was certified (by Drs. R. C. Mullins and G. C. Purvis) on 2nd and 3rd August, 1905, at the age of 27 as recurrent melancholia and was discharged 'relieved' 6 mo. later. It is not known whether there were any children. In any case no "Bruce" evidence is available as to the length of time Luke stayed with them in Belmont Valley. The Hospital records say "a week's trial." Rupert Smith says one month.

If this is correct, the date of the photograph showing the exodus to Thorn Hill would have been September 1904 (or early in 1905, if Bruce returned him to the Mental Hospital as alleged). [The Smith baby in the photograph is Amy, born 1904 and apparently under one year old.]

If one may refer to interviews with the two apostles of the legend, Mr. W. Muscott said "I never believed the baboon story, but it had good news value and will

in time become the classical African story." He states that he wrote the *Outspan* story, "The truth about the 'Baboon Boy,' " which appeared over the signature of George Harvey Smith, on 4th September, 1931. Rupert and Ted Smith agree that the language is quite beyond the capacity of G. H. Smith, who supplied the facts only.

Mrs. Ethelreda Lewis is mainly desirous of saving Luke from exploitation. She was misled by the Smiths on two points. Lucas had a name in Burghersdorp and could then talk English. Mrs. Lewis says he must have lived somewhere, which is a truism. She also says that a tale does not originate without some basis. The basis could equally have been a local joke elaborated in the hope of gain.

Ted Smith states that a neighbour (L.K.) told the Smiths that Luke would be worth 35,000 Pounds to them at Hollywood. Mr. Muscott was in touch with a New York Film Firm (21st October, 1930 to 14th January, 1931) who were working a jungle serial, but the negotiations fell through.

G. H. Smith's attempt to exhibit Luke as the baboon boy at East London was a fiasco. He just cleared expenses (Rupert Smith). There was a disturbance (Mrs. E. Lewis) and the police closed the show (A. Hobley).

The difficulty in obtaining first hand evidence of happenings in 1904 is increased by the death of so many of the witnesses. These include Dr. James Tandy Bolger of Burghersdorp; Dr. W. L. A. Leslie, later of Tsolo; Mr. George Smith, Sr., night attendant; Head attendant Walters; Tom Wesson, male attendant; Male nurse Bruce and Mrs. Bruce; J. Lewis Smith, attendant (killed, 1910); George Henry Smith, his employer; Stephen Wallace Colegate; Sergt. Chas. Holsen (died of pneumonia in 1924); Sergt. J. P. Venter (died of dropsy at Port Alfred, 1936).

Mrs. George H. Smith still employs Luke, who has been a cheap and valuable servant. She denies all knowledge of the history and was obstructively reticent and grudging of information.

Dr. J. A. van Heerden found Luke unable to pass the moron tests and will report on his defective mentality.

To sum up my own opinion, Luke or Lucas was a normal half-bred boy until his brain was damaged by the accident which fractured his skull in childhood.

The arrest to his development has not been helped by his later history and environment.

The story is his bullying by persons named by Ted Smith and the suggestions enforced by hot cigarette ends, to my mind explain any story about baboons which Luke is supposed to have told.

I can find no evidence, beyond the story ascribed to Sergt. Charles Holsen, to support the belief that Luke ever lived with baboons, even if the police found him in the neighbourhood of a troop of the animals.

(Signed) E. G. Dru Drury
10th January, 1940

Const. W. J. Coetzer, No. 9563, engaged in tracking stock thefts, interviewed on Tuesday, 25th October, 1939, said:

"I was born in Alice District and speak Kafir. In 1921 I was sent from the Depot in Pretoria and stationed at Spring Mount, Alexandria. Sergeant Charles Holsen was in Command there. He told me the story of the boy with the baboon troop. He did not tell it more than once, but when I came to be stationed at Kafir Drift in 1926 we used to get our horses shod at Smith's. I saw the boy and recollected Holsen's tale. I heard it also from George Smith. I saw the boy catch and eat a cricket in the forge.

"Holsen and Sergeant J. P. Venter certainly believed the tale and were not the sort of people to make up a yarn. He described the spot as the flats on the Bathurst side of Kafir Drift. The native story was that years before, in the Peddie side of the Fish River, natives saw a baboon carrying off a baby. Native superstitions would prevent their claiming the recovered boy. The whole family might have been murdered as witches.

"George Jesse, an ex-native policeman, heard the whole story from an old native, now dead. He will try to recall the name. There is a reliable headman in Mbokatwana location on the Peddie side of the drift whom I will see. He can perhaps help.

"Holsen said that they kept the boy for some weeks at the Trapps Valley Station and, finding him speechless, sent him in to the doctor for examination.

"Holsen died in the Grahamstown Hospital in 1924. He came in for a musketry course and got pneumonia.

"Venter died two or three years ago.

"Holsen belonged to the old C.M.P. He did not patrol outside the Bathurst District. I do not know whether he was ever in the Burghersdorp area."

(Signed) E. G. Dru Drury

RECORDS FROM THE ARCHIVES OF GRAHAMSTOWN MENTAL HOSPITAL

Case record of Lucas. Admitted 30/3/04; registry number 2679. From Burghersdorp. Age 13 years. Kafir. First attack—duration 2 mo. Single; herd boy. Cause, injury to head. Weight 56 lb. Certified by Doctors Caiger and Bolger in February, 1904. "Foolish and nervous manner. Laughs without cause and tries to stand on his head. He is destructive to his clothes and dirty in habits. Said he was a goat, also that he was a sheep."

History of case. He was admitted to the Burghersdorp gaol as an indoor pauper, nothing being known about him or his people. While in gaol the Gaoler reports that he was destructive and behaved like a lunatic, though quite harmless.

Condition on admission. Little "niggar" boy with very mischievous expression. Very emaciated and having a large semilunar indentation over the left side of the skull for about 5 inches from tip to tip. This, he says, is the result of a kick from an ostrich. Right tibia shows a badly united fracture.

Notes. (31/3/04): "Quite happy, laughing and saying this is a very comfortable place. His temperature is normal today and he eats ravenously anything within reach." (7/4/04): "Very bright youngster, but rather incoherent in his replies to questions. He is in good health, eating and sleeping well." (1/5/04): "Remains the same. In good health and spirits. (10/5/04): Lucas has gone on a week's trial to stay with Attendant Bruce. Being the youngest member on the Native side and inclined to be very cheeky and impertinent to his elders, he was continually in danger of being assaulted." (15/5/04): Discharged as recovered, to remain with Attendant Bruce. The diagnosis was "Acute mania" and he was neither epileptic, suicidal, nor dangerous.

(Copied by Dr. C. G. Cassidy)

SOUTH AFRICAN POLICE

Office of the Deputy Commissioner
Cape Eastern Division
Grahamstown, South Africa
1st March, 1940

Dr. E. G. Dru Drury
High Street
Grahamstown

(Enquiry re Baboon Boy—Lucas)

Sir:

With reference to our conversation relative to the above matter, I have the honour to inform you that Constable Coetzer has again been seen by me. He informs me that the late Sergeant Holsen definitely told him that he (Holsen) caught this native as described by him in his report of the 29th April, 1939.

Full facts of the herd boy Luke and the Basuto shepherd called "August" certified at Burghersdorp and later admitted at Grahamstown were explained to Constable Coetzer. He states that although Holsen was a very truthful man, he (Coetzer) now feels convinced that "Lucas, the Baboon Boy" is identical with Lucas certified at Burgersdorp and later admitted at Grahamstown.

In my report of 1st May, 1939, I stated that there is no reason to believe that the story of finding of the "Baboon Boy" by the Police is not authentic.

At that time I based my belief on the facts then revealed, but considering the evidence now brought to light, I am definitely inclined to the view that the boy Lucas certified at Burghersdorp is identical with Lucas the supposed Baboon Boy.

I have the honour to be,

Sir,

Your obedient servant,

(Signed) O. J. T. Horak (Lieut-Colonel, Deputy Commissioner
OJTH/SC S.A. Police Commanding Cape Eastern Division)

A summary of the evidence in the case of Lucas reveals the following pertinent points.

(1) According to report, Lucas was captured by two troopers of the Cape Police; according to documentary evidence, Lucas was in the Burghersdorp gaol at the time of his reported capture.

(2) The Bathurst side of the Kafir Drift, the place of his reported capture, and Burghersdorp, where he was incarcerated as an indoor pauper, are widely separated. It would have been impossible for Lucas to have shuttled between the two places.

(3) According to report, the officers making the capture took Lucas to the Grahamstown Mental Hospital; according to documentary evidence, Lucas was transferred to the Grahamstown Mental Hospital from the Burghersdorp gaol.

(4) The story of Lucas's feral existence was not current at the time he was at the Hospital. It did not appear upon the Hospital records and it was not known by report among the attendants or other patients.

(5) The story appeared after Lucas was given into the care of George H. Smith, who attempted to exploit the tale by exhibiting Lucas as a "b on-boy." It was, therefore, to Smith's interest to promote the story—which he did vigorously.

(6) The retardation of Lucas and much of his anomalous behavior may be accounted for from a brain injury over Borca's speech area, which is indicated by a 5-in. semilunar fracture in the skull—probably caused by the kick of an ostrich. The present Lucas of Smith's Thorn Hill Farm is positively identified with the hospital records by a recent x-ray of the old skull fracture.

Dr. Foley's note on "The 'baboon boy' of South Africa" was accurate for the time it was written. Its publication, however, was precipitous, as the recent evidence clearly shows. The Lucas case indicates how thorough investigation must be in matters of this kind, and how cautious and conservative scientists should be in accepting and in sifting the evidence for feral man.

University of Denver ROBERT M. ZINGG

THE ELEVENTH ANNUAL MEETING OF THE EASTERN PSYCHOLOGICAL ASSOCIATION

The eleventh annual meeting of the Eastern Psychological Association was held at Chalfonte-Haddon Hall, Atlantic City, on April 5 and 6, 1940, with an attendance of 356 members and 199 guests, making a total of 555. While the total attendance was more than 100 below the number attending the meeting at Bryn Mawr in 1939, several compensating factors must not be lost sight of. Since this was the first time that the annual meeting was held away from a collegiate institution and a large center of population, the attendance must be regarded as very satis-

factory indeed. On the other hand, the number present at the Presidential dinner, 270, was the largest on record for this function. It should also be noted that in spite of the slight reduction in attendance, the number of dues-paying members and guests, hence the income of the Association, was greater than in any previous year. The total income amounted to $630.57 which leaves a net surplus of $231.16 for the year. Enthusiasm and interest were high and many members expressed a preference for meetings at a hotel such as this one was. Taking everything as a whole, the Association must be regarded as having made progress during the past year while trying out the new experiment of meeting under unusual circumstances and in a different environment.

There were 17 sessions featuring 109 papers, each paper having 15 minutes for presentation and 5 minutes for discussion. On Friday morning 37 papers were given in six sessions under the following topics: Methodology and Statistics, Applied Psychology, Cutaneous and Auditory Processes, Physiological Psychology, Social Psychology, and Perception and Memory. In the afternoon there were five sessions with 32 papers in the following categories: Brain Function, Conflict and Motivation, Visual Processes, Tests and Measurements, and Films. Under this last rubric were included papers requiring films and a number of instructional films produced at Columbia University by C. J. Warden, T. A. Jackson and G. M. Gilbert. Sandwiched in between regular sessions and the dinner hour late Friday afternoon were six Round Tables which are becoming increasingly popular at the meetings: Selection and Training of Civilian Airplane Pilots, J. G. Jenkins, University of Maryland, Chairman; The Effects of Frustration, A. H. Maslow, Brooklyn College, Chairman; The 1937 Revision of the Binet Test, R. T. Rock, Fordham University, Chairman; The Psychology of Social Change, Ross Stagner, Dartmouth College, Chairman; The Licensing of Psychologists, R. C. Challman, Columbia University, Chairman; and The Census from the Standpoint of Social Psychology, J. B. Maller, New York University, Chairman.

After a meal which all agreed was really a 'banquet' the President of the Association, F. L. Wells of Harvard University, was introduced by the previous President, K. M. Dallenbach of Cornell University. The subject of President Wells' address was "Mental measurement and college objective." The address threw new light on college objectives both from the point of view of officers who formulate them and the students who are subjected to them. It was pervaded with a quiet humor that took the sting out of some of the facts brought to the attention of the audience. Since the address is to appear in print no attempt will be made here to give a better idea of its contents.

Six sessions in which 40 papers were given were held Saturday morning on the following subjects: Abnormal Psychology, Learning and Conditioning, Child Psychology, Social Attitudes, Personality, and Brain Function.

Owing to the need for time to consider the new by-laws, which were prepared by a Committee for Revision of the By-Laws, the general session was omitted and the whole of Saturday afternoon was allowed for the annual business meeting. The following business was transacted: a budget amounting to $510.00 was adopted for the year 1940-41 with an amendment that if it is necessary to exceed this budget in the execution of the new by-laws the Directors may authorize necessary increases in expenditures; a motion was passed supporting the stand of the New York City Board of Higher Education in its appointment of Bertrand Russell to a Professorship

of Philosophy in City College; another motion bearing on this questoin was then passed according to which no publicity shall be given to the motion supporting the Board of Higher Education without a statement of the total membership of the Association, the number attending the business meeting, the number voting for the resolution and the number voting against. There were 75 present at the business meeting, with 37 voting in favor and 23 against the Russell motion.

Before taking up consideration of the new by-laws, Douglas Fryer of New York University, Chairman of the Committee on Revision of the By-Laws, read a paper entitled "The new by-laws in relation to our history." In his address Professor Fryer gave an illuminating account of the origins, problems, development, and present status of the Association backed up by statistics pertinent to the chief points of interest. The members present at the business meeting voted unanimously to accept the recommendation of the Directors that the new by-laws be adopted in principle for one year with final action to take place at the next annual meeting after making any detailed changes necessary to translate the by-laws into action. The chief changes in the new by-laws are as follows: membership is limited to properly qualified individuals who are not more than two years in arrears in dues; guests will pay the same fee that members pay in dues (with certain exemptions for relatives of members); the Nominating Committee is eliminated and members will henceforth present one candidate each for President and two members of the Board of Directors on a nominating ballot, and will vote by preferential ballot for a certain number nominated for each office in a second mail ballot.

New officers were elected as follows: W. S. Hunter, Brown University, President, 1940-41; Harry Helson, Bryn Mawr College, Secretary-Treasurer, 1940-43; Elmer Culler, University of Rochester, and Irving Lorge, Teachers College, Directors, 1940-43. The Program Committee appointed by the Directors for 1940-41 consists of D. G. Marquis, Yale University; L. Max, New York University; and R. A. McFarland, Harvard University, Chairman.

With the passage of motions thanking Douglas Fryer for his work as Chairman of the Committee on Revision of the By-Laws, the Secretary-Treasurer and his staff for their work at the meetings, and the management of Chalfonte-Haddon Hall for the excellent facilities and handling of the various matters connected with the meetings, the business meeting came to a close. The next annual meeting of the Association is to be held at Brooklyn College on April 18 and 19, 1941.

Bryn Mawr College HARRY HELSON

BOOK REVIEWS

Edited by JOHN G. JENKINS, University of Maryland

American Psychology Before William James. By JAY WHARTON FAY. New Brunswick, N.H., Rutgers University Press, 1939. Pp. x, 240.

Everybody says that James and Ladd and Hall imported into America the "new psychology" from Germany and that before them there was no American psychology. Such an assertion is, of course, improbable on its face, for there must have been in the American colleges and universities some discussion of the philosophy and nature of mind. Mr. Fay set himself to resolve this contradiction, to find out just what psychology had been thought and taught and wrought in America from Henry Dunster in 1640 to William James in 1890; and an excellent encyclopedic job he has done! All the facts are now in his book—that is to say, there are enough of them in exact quotation, in summary paragraph, in explicit biographical note, in precise commentary on bibliographical citation, to enable the careful student to cull the data, to find his way to the literature, or to reconstruct the total picture adequately.

Whether the picture turns out to be dreary or engrossing depends upon the reader. Mr. Fay is absorbed by it and champions the neglected Samuel Johnson, Thomas Upham, and Noah Porter, attempting to rescue them from the oblivion in which they have been unjustly immured. That is right, for an author should be loyal to his subject; but I, who am interested in the past, not, be it admitted, for its own sake, but because it leads to the present—I found the book chock full of information and utterly dull. The past does not necessarily lead to the present even though it come first; dynamic relations between successive events have to be positively established if the late is to be explained by the early. So we have the right to ask: What did James and Hall owe to Porter and McCosh? I still am convinced that James and Porter or Hall and McCosh are as incommensurable as were their European prototypes, Helmholtz and William Hamilton.

Mr. Fay's book, after despairing of any modern's understanding the Puritan mind, treats of two and a half centuries in five periods. (1) The first is the period of scholastic education after the English pattern (1640-1714), for which the author quotes from the note-book of Samuel Johnson, then an eighteen-year-old tutor at Yale, to show what American academic psychology was like in the days before the arrival of Locke's *Essay* from England. (2) The second period (1714-1776) dates from the arrival of the *Essay* and shows Samuel Johnson quoting, practically verbatim and at length, from it in 1752. Meanwhile Bishop Berkeley himself had been in America for two years and stimulated beliefs in empiricism and idealism. Jonathan Edwards and Thomas Clap were the other important Americans who spread this influence. (3) Next came the period (1776-1827) of Scottish philosophy, of faculty psychology, followed (4) by the "era of American textbooks" (1827-1861). Scottish psychology, Mr. Fay says, was diluted, after fifty years of supremacy, by an admixture of Cousin, Kant, and Coleridge. Important texts were published, as, for example, by Upham (1827), "the Sully of America." Benjamin Rush started abnormal psychology (ca. 1812). (5) Finally there was the period of British and German

philosophical influence (1861-1890), where the important names are James Rush (Benjamin's son), Noah Porter, James McCosh (Scottish in person and thought), and B. P. Bowne. They knew their minds and selected their influences. It was not Darwin and Galton in England nor Helmholtz and Wundt in Germany who fired their imaginations!

The Notes are forty-seven pages of fine print to which is added a useful eight-page chronological table. Although they are inconvenient, being separated from the text and yet not making sense without the text, they have in them a mine of biographical and bibliographical information and exhibit clearly the author's careful and unwearied scholarship.

Mr. Fay does not wholly ignore the dynamic relationships of historical evolution. He exhibits with assurance the effect of Locke and the later effect of Reid, and he traces both influences along their courses. At other times, however, he seems to me to mistake sequentiality for causality, or at least he implies too much. Take these sentences: "Behaviorism did not spring Minerva-like fully grown from the brain of J. B. Watson. It was announced half a century before by his John the Baptist, James Rush" (p. 145). In addition to the difficulty of imagining Watson simultaneously as the Chief Olympian and the Prince of Peace, there is the confusion that arises from the simple fact that all sorts of objectivists, besides Rush and better known, anticipated Watson, who was himself an excellent founder of a school and therefore—on the view that movements can not be founded until after they have been prepared—not so good an originator.

To Mr. Fay's horrification—there could be no doubt about it—I venture to suggest that one important use for his book is to make some ignorance safe. One is never safe in being ignorant unless he knows what he is being ignorant of. Mr. Fay tells us what we do not need to know. His is a book to own on the shelf and use at need

What, t.. .., has become of the thesis of this book, that all this American psychology, from Samuel Johnson to McCosh, being the antecedent of modern American psychology, is also its partial explanation? Can wisdom be complete if ancestry be ignored? Well, Mr. Fay does not convince me that he has got the genealogy right. "American Psychology Before William James" died, I think, without issue. Living in the old house are some immigrants of mixed blood, who stem in part from Darwin and in part from Helmholtz or from their kin. These immigrants have even moved in their own furniture.

Harvard University EDWIN G. BORING

Introduction to Psychology. Edited by E. G. BORING, H. S. LANGFELD and H. P. WELD. New York, John Wiley & Son, 1939. Pp. xxii, 652.

Although the present volume is essentially a revision of *Psychology: A Factual Textbook,* it has undergone such major changes that it merits detailed reconsideration. No one will quarrel with its premise that the "Facts should be presented to the young student of Psychology free from the bias of metaphysical presuppositions or of psychological systems," yet one can not escape the conclusion that, at the present stage of psychology, such bias is intrinsic to a presentation of the material. An author may, for pedagogical reasons, omit theoretical discussions and avoid controversial points, but whether he states and discusses his systematic and meta-

physical biases he uses them in the selection of his facts for presentation, and in his relative emphasis upon various sections of the subject matter.

In the *Introduction to Psychology* the most striking systematic note is the strongly social or sociological point of view which is emphasized by an inversion of the order of presentation from that of the earlier book. The change was made partly in response to a popular demand and partly for pedagogical reasons. Psychology becomes, now, the search for an answer to the question "What is man?" The program as outlined in the first chapter is to study "persons," first in their "social relations" and then as "single individuals." The second chapter deals with "social behavior," the third with "personality," the fourth with "individual differences," and the fifth with "motivation." At this point the evaluative attitude gives way somewhat to the study of the data of experience and of physiology. "Emotion and Affective Responses" are discussed. Interest in them derives from a consideration of "needs," in which physiological labels already have been utilized, but the treatment remains interpretive. With physiology thus introduced, a detailed consideration of the "Response Mechanism" and of "Response" in general follows. Then "Learning" and related forms of behavior, such as "Retention," "Recollection and Imagination," and "Thought" are discussed in that order. The final third of the text is devoted to "Perception" and "Sensation." "Perception is the apprehension of immediately present objects and events," and as a consequence, the chapters present many disparate items of that type of knowledge without offering any unifying concepts of integration or organization of descriptive data. "Sensation is the core of perceptions. It is what the organism would perceive if it had only the nature of the stimulus and the stimulated sense organ to go upon." The nature of the stimulus is analyzed in some physical detail for each sense department. Likewise the anatomy of the sense organs is described and expositions of the associated physiological hypotheses are given.

The "modernistic approach" involved in the rearrangement of topics and in the emphasis upon social interpretations follows certain trends of current psychology, but as Fernberger has pointed out, such trends run in cycles in psychology as elsewhere. The impression one gets of the treatment as a whole is strongly reminiscent of James, in that it begins with gross phenomenal entities—society and man—and leads toward simpler phenomenal entities of the same order—responses and sensations. "Sensation," now as then, is an epistemological unit, the simplest item of knowledge. Whether it has ever been anything else is arguable.

Although teachers of psychology seem to have found it too matter of fact and lacking in "thrill" to offer student "appeal," the concept of sensory data comparable with the data of other sciences has been experimentally fruitful. Data ordinarily form the material out of which scientific systems are built by interpretations under the laws of logic. When data are relegated to the end of the discussion, however, they lose this function. It is not surprising, therefore, that much "of the scientific details of those fields which are today richest in established facts" has been omitted. Modernization, it seems, has reversed the order of reasoning so that we go, now, from interpretation to facts—but then the facts seem of less significance. Hence Chapter 20 consists of one paragraph the thesis of which is "Da Capa!"

We have not forgotten, however, that the major purpose of a text book is pedagogical. It may be that systematic sacrifice is justified on this ground. There is no certainty that social concepts are closer to an individual's experience than are

concepts of experiential data, nor are complex social values necessarily easier to comprehend than simpler descriptive ones. It is interesting, therefore, that the temper of the times with respect to psychology should deviate so markedly from the accepted procedure of the other natural sciences. One wonders what would be the reaction to beginning the study of elementary physics with electronics, or of elementary chemistry with the structure of colloids.

A danger that the reviewer sees in introducing the study of psychology with a discussion of "Social functions of the Individual" is that the language one must use is replete with words that have significances different for the student from those intended by the writer. The author of Chapter II is fully aware of the danger but it is difficult to see how it can be avoided without labored definition. The alternative device of cross-reference (on page 23 "see page 275 f." or on page 33, "see page 498") is annoying, inconvenient, and probably useless, if a response is expected from the student. The section on "Reification" might well have been studied and applied more carefully by the authors themselves. They are guilty of reification on many occasions, and there is little in the book's attitude of social, personal evaluation that will force students to distinguish between their reified opinions and facts.

In the hands of those instructors for whom it was written, this *Introduction to Psychology* will find ready acceptance. There is no question that the writers of the several chapters on the social and applied aspects of psychology, which set the tenor of the book, have given adequate presentations of their material. Students will tend to be intrigued by the apparent utility of the evaluations. Whether they will understand themselves or learn how to live better is not so certain. On the other hand, if some of he "social psychologists" can be induced to follow its rational treatment of social behavior much may be accomplished.

Conservatives, reactionaries, and some who have fought for psychology as a natural science will be disappointed with this product of so many of the influential laboratories of psychology. Both the systematic implications and the pedagogical results are less than might have been hoped.

Hobart College FORREST LEE DIMMICK

Collective Behavior. By RICHARD T. LAPIERE. New York and London, McGraw-Hill Book Co., 1938. Pp. xiii, 577.

This book is designed as a "second" course in social psychology. Emphasis is placed on "social interactions themselves" rather than upon "the processes by which individuals develop their personalities"—a distinction which left me as cold and mystified at the conclusion of the book as it found me at the commencement. Why is this a "second" and not a first course? There is nothing more difficult about this text or its matter, nor more sequential to or dependent upon prerequisites, than happens to be the case with any treatise on social psychology, which is very little indeed—so little, in fact, that it would be sheer humbug to pretend that it contained anything that a freshman of *IQ* 100 couldn't assimilate immediately, with no more than ability to read elementary English.

The reviewer fails, however, to 'resonate' to this book for reasons of a more disturbing kind, stemming from the gratuitous distinction above, which draws so much water for LaPiere. The time seems long past when it was considered profitable to regard personality in a vacuum and social interactions by "themselves," whatever that may be. Personalities interact and thus develop, so that the resulting personalities

and modes of interaction, never separable, reciprocally modify one another. What else is there to personality, once the individual has started with the slight hint from his genes? With the best will in the world this distinction between "social interactions themselves" on the one hand and determinants of personality on the other must be regarded as a manifestation of the sociologist's chronic reification of "social processes," as something above and beyond the individual, as an independent entity like the cat's grin in *Alice in Wonderland*. It may be forgivable as a convenient schematization, provided the student be cautioned once to a page that it is no more, but in LaPiere no evidence is given of the recognition of such discrimination as sheer artifice that does violence, perhaps a necessary violence, to the facts. This naïveté is fully the equal of that of the physical scientist who sees in water no more than oxygen and hydrogen. If this book stands for anything systematic, it stands for the rehabilitation of the group-mind notion, despite LaPiere's pious disavowal, but now in the trappings of the more sophisticated and verbally less occult guise of "social interactions." Aware of his vulnerability in this respect, LaPiere does some shadow boxing in which he gives himself the decision. He accuses those who regard the individual as the sole and ultimate repository of social reality of throwing the baby out with the bathwater, of destroying "the body since they do not like the soul imputed to it." He accuses them of denying the very existence of collective behavior or social interaction. Such denial is news to the reviewer. What is denied is that it can remain as an entity, like the grin of Alice's cat after the cat was out of sight. Behavior is always individual and can be nothing else. In isolation it is one thing. In a social context it is another and different thing, because the determinants are different, but it is still individual. That there should be the slightest opportunity or occasion for such a discussion as mine above suggests that the group-mind controversy, far from being a "thing of the past in American social psychology," as LaPiere avers, has preserved a perennial vitality, among sociologists at any rate.

LaPiere commends his book for its logical cogency and unity, but it is an externally imposed unity that makes for neat dialectics but doubtful psychology. This unity consists of the rigorous application of a formula of treatment to each type of social interaction. For example, the type "Conventional Behavior" is analyzed into the nuclear topics of *Origin and Function, Ideologies, Membership, Overt and Covert Aspects of Interaction,* and *Leadership*. Precisely the same formula of analysis is applied to "Regimental Behavior," "Formal Behavior," "Congenial Behavior," etc., with relentless insistence.

Beyond establishing a rigorous formula, however, this logical cogency sometimes breaks down sadly. In discussing conversional audiences (p. 258), LaPiere, influenced apparently by the heavy weight which economists place upon "marginal utility," feels that he has here a key concept of the utmost importance, for it is, according to him, the "marginal listeners" of an evangelist who determine the fate of his movement. The analogy to "marginal utility" is so close as to be painful to the critical reader. At any rate, LaPiere holds that the primary concern of the leader is not with his followers, who are already loyal, or with his antagonists, who are in opposition, but with the "marginal listeners," who are indifferent and can turn the issue in his favor if they are converted. Unless LaPiere has a very original notion as to the number of such "marginal listeners" in any really important movement, the idea sounds as if the Christian conversion of the Roman Empire had never occurred,

a Hitler had never appeared, and a Roosevelt had never been elected. Was the Christian conversion, the rise of Hitler, or the election of Roosevelt—or for that matter any considerable social transformation—the result of the slight addition of weight from the side of the indifferent, or of the conversion of a far greater number of the active opposition? For want of actual figures, we prefer to venture the guess that disgruntled pagans, Social Democrats, and Republicans had a determining effect on the results. Inconsistently enough, LaPiere too seems to agree with this view (p. 258), only he calls such converts from the opposition also "marginal." The question is, then, when is a listener "marginal"? When he's initially indifferent? Yes, says LaPiere—and then proceeds to include the originally hostile. Obviously, conversion is meaningless if it can affect only the initially indifferent. For effectiveness in social transformation it must succeed also in drawing in the hostile, who can be won if the leader has the right code of appeal, just as his adherents can be lost if he has the wrong one for them at the present moment. Thus the matter involves a complex total pattern in which initial adherence, hostility, or indifference are merely components among others.

University of Tampa ELLIS FREEMAN

Moses and Monotheism. By SIGMUND FREUD. Translated by Katherine Jones. New York, Alfred A. Knopf, 1939. Pp. 218.

The Hebrew Moses: An Answer to Sigmund Freud. By TRUDE WEISS ROSMARIN. New York, Jewish Book Club, 1939. Pp. 64.

Every new book of Freud's not only repeats but also modifies, and sometimes clarifies, his fundamental theories. His last book, properly a supplement to his *Totem and Tabu* of 1912, is no exception. Now that Freud is dead, a special interest attaches to his last word.

It is nonsensical, says Freud, to maintain that psychoanalysis is practiced if the early periods of life are excluded from one's investigation; yet this claim has been made in many quarters (p. 114). Traumata belong to early childhood, the period up to about five years. Impressions during the time when the child begins to speak are especially interesting. The period between two and four years is the most important (p. 116). The strongest obsessive influence derives from those experiences which the child undergoes at a time when his psychical apparatus is incompletely fitted for accepting them (p. 199).

Childhood traumata concern impressions of a sexual and aggressive nature and also early injuries to the self (injuries to narcissism). It is very striking that the sexual factor should predominate. Analytic work alone can yield a knowledge of the forgotten experience, or—to put it more concretely, though more incorrectly—is able to bring those forgotten experiences back to memory (p. 116 f.).

Human sexual life shows an early blossoming which comes to an end at about the age of five years. Then follows a period of latency. This suggests that man is derived from a species of animal that was sexually mature at five years, and arouses the suspicion that the postponement, and the beginning twice over, of sexual life has much to do with the transition to humanity. Man seems to be the only animal with a latency period and delayed sexuality. Investigations of primates would furnish a test for this theory. Perhaps this state of affairs is a necessary condition for the existence of neurosis, which seems to be a human privilege (p. 117 f.).

The effects of the trauma are twofold, positive and negative. The former are endeavors to repeat the traumatic experience. If it was an early affective relationship it is revived in an analogous connection with another person. These endeavors are summed up in the terms 'fixation to the trauma' and 'repetition-compulsion.' The effects can be incorporated into the normal ego and lend to it immutable character traits. The negative reactions are defensive, culminating in inhibitions or phobias. These negative reactions also contribute to the formation of character. Actually they represent fixations on the trauma no less than do the positive reactions. The symptoms of neurosis constitute a compromise. The opposite reactions create conflicts (p. 118 f.). Neurosis may be regarded as an attempt at cure (p. 122).

A partial return of the repressed takes place when these conditions are present: an illness which acts on the ego, sleep, puberty, a new trauma similar to the old (p. 150), new problems of life (p. 121).

The topography of the psyche (id, ego, super-ego) has in general nothing to do with cerebral anatomy; there is only one point where it impinges on it (p. 153).

The theme of *Moses and Monotheism* is that religious phenomena are to be understood only on the model of the neurotic symptoms of the individual, as a return of long-forgotten important happenings in the primeval history of the human family (p. 88). The analogy with psychopathology is very complete, approximating to identity, in the nature of an axiom (p. 113 f.). In spite of the attitude of biological science, which rejects the idea of acquired qualities being transmitted to descendants, Freud cannot picture biological development proceeding without taking this factor into account (p. 158). "Men have always known . . . that once upon a time they had a primeval father and killed him" (p. 159). From this ancestral trauma of father-murder religion stems.

On this basis Freud reconstructs human history in six phases: a traumatic period of father-murder, totemism and mother-right, humanization of God and father-right, Judaism, Christianity, and Islam. The last three monotheistic phases are represented as stages in the 'return of the repressed.' In the case of the Jews, the return of the repressed was precipitated by a new trauma similar to the old, the killing of Moses.

With this Freud enters the difficult field of Biblical criticism, a field in which even experts more often go wrong than right—and Freud is no Biblical expert. The one time he uses a Hebrew word he confuses it with a German word (p. 180). When arguing that Moses was an Egyptian, Freud neglects to mention that Moses is called an Egyptian in the Bible (Exodus, 2, 19), although the context, it is true, robs the passage of probative force. In assuming the murder of Moses, Freud accepts the opinion of a German scholar, an opinion based upon the emendation of an obscure verse in Hosea. No other scholar has accepted the emendation. *The Hebrew Moses, an Answer to Sigmund Freud,* is an expert's answer to Freud's Biblical venture.

It certainly should not be beyond the ingenuity of psychoanalysis to explain the indubitable 'paternal' elements in religion without assuming ancestral traumas and the historical killing of fathers and father-substitutes.

Freud's concept of repetition-compulsion merits the consideration of general psychology. Repetitive urges are a prominent feature of childhood. In connection with the controversy over the rôle of repetition in learning, it is pertinent that repetition, or self-imitation, is the child's natural mode of practice. A repetitive urge is also found in the tendency to resume an uncompleted task. The efficacy for recall

of an 'intent to learn' is to be related to the fact that such a future intention gives the learning task an 'unfinished' character and thus, as has been proved by experiment, enhances its reproductive value.

Cornell University S. FELDMAN

Studies of Abnormal Behavior in the Rat. By NORMAN R. F. MAIER. New York, Harper & Bros., 1939. Pp. 81, 16 plates.

This monograph is an account of Maier's investigation of experimentally-produced neurotic attacks in the rat which won the thousand dollar prize awarded by the American Association for the Advancement of Science at the Richmond meetings. What is a neurotic attack? It consists of an active phase, which is characterized by hyperactivity, convulsions, hopping movements and tics, and a passive or coma-like phase during which the animal manifests various degrees of waxy flexibility. The similarity of this pattern to an epileptic attack is noted by Maier.

Maier employed the following procedure for inducing neurotic behavior. A rat was trained on the Lashley jumping apparatus to discriminate between a 'correct' design, *i.e.* one which designated the opening to food, and an 'incorrect' design, *i.e.* one which if jumped to did not give way and consequently dumped the animal into a net below the opening. After the discrimination was established, changes in the original pair of stimulus-cards were made so that the rat no longer had available cues upon which to base a choice. The rat, thereupon, resisted jumping. By applying either an air-blast or an electric shock to the animal, the jumping reaction was forced.

At this point, either before or after the animal had jumped, neurotic attacks made their appearance, but only to the air-blast and never to the shock. Maier explains this difference on the ground that the shock is something which can be avoided, whereas the air-blast cannot be. A test of this hypothesis could easily be made by having the air-blast turned off as soon as the animal jumped, just as the electric shock as a stimulus ceases upon the rat's leaving the platform.

Recent work by the Morgans at Rochester and in the Western Reserve Laboratory suggests that the labor involved in training rats to make discriminations (and anyone who has used the jumping technique will appreciate the use of that word 'labor') in order to obtain neurotic attacks is unnecessary. Attacks of a very severe kind can be obtained by placing the rat in an enclosure and releasing compressed air through a nozzle for about two minutes. Maier, himself, describes one attack to the air-blast when the rat was not in the experimental situation. Maier doubts the efficacy of hissing air alone, since in an experiment with six rats, in only 3 out of 63 exposures to air alone did attacks appear whereas there were 27 attacks out of 67 tests using the air-blast in the discrimination situation. The details by which the adequacy of this experiment might be judged are not available in the present monograph. Even though a conflict situation may evoke more attacks, the observations of Maier and others indicate that conflict is not a necessary condition for evoking the neurotic pattern. A hissing noise alone is an adequate stimulus.

Maier distinguishes between nervous and neurotic behavior. A rat may show marked fear reactions and yet not 'break down.' The reviewer suggests the hypothesis that nervous behavior may actually protect the animal against attacks. Studies in progress at Western Reserve indicate that it is the phlegmatic rather than the emotional rat which is more likely to manifest an attack.

The reader will find no final answers in Maier's monography. He will, however, find himself asking many significant questions and hurrying to the laboratory to find the answers (we hope!). The reviewer contended several years ago that the comparative psychologist might be of real service to the abnormal psychologist. This monograph is an important demonstration of the truth of that contention.

A word as to the organization of the book. After a short introduction and general description of the method, there follow rather detailed case histories of ten neurotic rats. These histories take up more than half of the book. The last 30 pages are devoted to a discussion of the results. There are 16 splendid full page photographs of different aspects of the rat's behavior.

Western Reserve University CALVIN S. HALL

Phantasie und Kunst. Edited by FELIX KRUEGER and ALBERT WELLEK. Neue Psychologische Studien, Volume 14, No. 1, Munich, C. H. Beck, 1939. Pp. 131.

This number of Felix Krueger's *Neue Psychologische Studien* contains two articles: the first, *Gefühl und Kunst* (Feeling and Art) by Albert Wellek, is theoretical; the second, *Beobachtungen an Zweiklangen in getrenntohriger und beidohriger Darbeitung* (Observations on intervals presented to the ears separately and to both ears simultaneously) by Hans Sandig is experimental.

Sandig's investigation was designed to test Krueger's theory that consonance and dissonance are complex qualities, immediately perceptible without analysis, and that difference tones are important for the formation of these complex qualities. Stumpf had sought to disprove Krueger's theory by the assertion that when the two tones are presented to different ears, no difference tones can occur, and yet consonance and dissonance are perceived as usual. Krueger, on the other hand, found that when the tones are presented to different ears, the character of consonance and dissonance shifts toward neutrality. Three sources of error in Stumpf's experiments were (1) air conduction, (2) bone conduction, and (3) the fact that the observers knew what intervals they were to hear.

In Sandig's experiments, the first two sources of error were entirely ruled out and the second reduced to a minimum. Sandig's experiments substantiated Krueger's theory. When presented to both ears simultaneously, the intervals had their normal musical effects. They were immediately perceived as consonances or dissonances, and the intervals were recognized without effort. When presented to the ears separately, the two tones seemed like unrelated noises without musical meaning.

Sandig showed that neither beats nor combination tones originate centrally. A physiological condition for their occurrence is the peripheral union of the primary tones. The latter, he found, is also a necessary condition for a definite, normal experience of consonance or dissonance.

Albert Wellek's article, *Gefühl und Kunst,* is an interesting attempt to resolve the conflict between two schools of esthetics, the formal and the expressive. He contends that the error of formal esthetics consists in setting up an opposition between form and feeling. Form, he points out, is no enemy of feeling, but its necessary vehicle. He contends that the essential mark of esthetic forms is *Gestaltiefe* (form depth), by which he means the significance of the forms as vehicles of feeling.

Whereas some esthetic theorists have thought that esthetic feelings were not real

feelings but merely "sham feelings," Wellek insists that they are genuine, and that they do not differ from the feelings of everyday life in quality, warmth, or intensity. They do differ in vital Ich-Nähe (ego nearness) and in their dynamics in the direction of overt behavior. The peculiarity of esthetic life is that esthetic feelings are shut off from practical activity. Just as a work of art is "framed" within the real world, so is the esthetic feeling framed. Children and naïve adults lack this frame of reference. That is the reason that art sometimes moves them to overt activity (such as shouting a warning to "Snow White" not to bite the apple).

Having established the criterion that esthetic feelings are shut off from overt activity, Wellek is logically forced to conclude that the feelings which drive the creative artist are *not* esthetic feelings.

In artistic creation, the feelings vary with the different stages of the work and with the personality of the artist. Often the artist feels as though he were being driven by a supra-individual stranger, who may be loved or hated or both at the same time. In certain stages of artistic creation, the feelings of the artist are eminently practical.

The highly personal spasms of amateurs have little or nothing in common with the work of real artistic creation, for the emotional significance of a work of art is strictly dependent upon its form, according to Wellek.

West Springfield, Mass. M. F. MARTIN

Spiritualità della Natura. By ALBERTO V. GEREMICCA. Bari, Laterza & Figli, 1939. Pp. 114.

Geremicca defends the thesis that the mechanistic interpretation in science may deny but can not destroy the possibility of spiritualism in nature. Mechanistic interpretation has become "official" in science. This orthodox viewpoint employs empty phrases and subtle sophistry in defending its position that man is a machine. Analysis is emphasized, synthesis is disappointingly neglected. When the arguments of the mechanists are reduced to fundamental postulates, mysticism results. Although it may be insisted that man is a machine, it must be remembered that every machine presupposes a machinist.

A case for spiritualism is presented by Geremicca. The existence of spiritualism might be denied at the present time, he states, but it can not be proved that spiritualism is excluded as a possibility in evolution. The theory of mutations teaches that equal forms may evolve into different things. There are, therefore, endless possibilities of variation. For example, "reading ability followed the development of perception in man . . . an outcome impossible of prediction." May not spiritualism result from an analogous variation? It is obvious, therefore, says Geremicca, that phylogenetic studies in science are indulging in phantasy when attempts are made to set limits to development.

In all of the four chapters (*Instinct, Heredity, Intelligence, Evolutionary Development*) Geremicca proposes and defends his argument that spiritualism can not be disproved. He writes with enthusiasm, clarity, philosophic orientation, and logical rigorousness. His words are nicely selected resulting in a style literary in quality. The book is scholarly rather than scientific in emphasis. The few references are drawn largely from biology. The reader may disagree with the thesis but he is nevertheless compelled to question his own habits of thought.

Psychological Test Bureau, Rochester, N.Y. ANTHONY J. MITRANO

Outline of Psychiatric Case-Study. By PAUL W. PREU. With a foreword by Eugen Kahn. New York, Paul B. Hoeber, Medical Book Department of Harper & Bros., 1938. Pp. 140.

This little manual is quite similar to one previously put out by Chaney several years ago. It offers little that is new in outlining psychiatric procedures. The book is composed essentially of two parts. The first part is an outline of the psychiatric history taking which is an outline in semi-tabular form of the essentials to be investigated by the psychiatrist who tries to elicit a past history of his patient. The usual subjects are gone into: family history, past personal history, and personality. The remainder of the book, except for short chapters on physical examination and other similar nonpsychiatric aspects, is composed of an outline of mental examination. The headings here are "appearance," "general behavior and attitude," "motor status," "stream of speech and mental activity," "content of thought," "emotional state and sensorium," "mental grasp and capacity." The book is to be commended because it is so complete, but it is to be condemned because of the lack of sample questions given to enable the psychiatrist to make a study of patients, who does not carry with him standard, exemplary replies to questions. In other words, the student or beginning psychiatrist picks up this book with the idea of carrying on a psychiatric examination is provided with an extensive prospectus on how the history and examination are to be made but is not provided with any means of evaluating the responses and reactions that he gets. A number of words are used in improper text such as the term "visual field" with respect to hallucinations, the term being more properly used with sensory examination of the eye. There is no glossary to explain the technical terms to beginners for whom the book is supposedly designed.

Detroit, Mich. LOWELL S. SELLING

The Psychology of Exceptional Children. By KARL C. GARRISON. New York, Ronald Press Co., 1940. Pp. iv, 351.

About one-fourth (5 chapters) of Dr. Garrison's book is devoted to a preliminary consideration of the origins of child behavior, individual development and differences, trait differences and education, and measurements in education (as well as in psychology). About one-sixth of the space (4 chapters) is given to a discussion of gifted children, slightly less than one-fourth (6 chapters) to mentally retarded children, and slightly over one-fourth (6 chapters) to children with visual, auditory, speech, and orthopedic defects, to children of lowered vitality, and to problems of left-handedness.

The student who is not familiar with the field will find here a brief summary on gifted and handicapped children, well written in the main in non-technical language, straight-forward, well organized, fairly well documented, and all-in-all sound in its main findings and conclusions. On the other hand, the experienced specialist on deviating children will find little in the book with which he is not already familiar. It contains practically no original contributions and it supplies little evidence that the author has had any firsthand clinical, educational, supervisory, or administrative experience with the types of deviates that are discussed.

The summary provided is indeed fairly satisfactory for a book of 340 pages, although isolated statements and conclusions occur that probably will not be accepted by some specialists in the areas concerned. References occur to a wealth of investigations consulted by the author. Many of the writers quoted, however,

have made only a single contribution and have not been closely identified with work among handicapped children. On the other hand, no reference is made to many significant studies (some more important than those cited), and a number of authorities who have for long published extensively and who for long have given all their time to the study of deviates, some as pioneers, are not cited at all, nor are their publications listed. In some cases literal quotations are made from such books in which the copyrights have not expired without the use of quotation marks or references (*e.g.* on pages 79 and 270 f.), although the author states that "in each case an attempt has been made to give credit by a rather complete citation."

Whether so much space should in a brief book be given to the material in Part I, which is readily available elsewhere, and much of which is not needed for the subsequent discussion, and whether a discussion of bright children should be included in a book dealing mainly with handicapped children are questions on which authorities may differ. The problems of the gifted and the handicapped are different problems. Usually those who work with the handicapped do not work with the gifted; similarly vice versa. It is doubtless true that the school administrator should know something authoratative about both the gifted and the handicapped, but it does not necessarily follow that the two should be presented in the same book, particularly when limitations of space preclude an adequate treatment of either group. With space saved in accordance with the above suggestions the treatment of the handicapped could have been profitably extended—*e.g.* to the inclusion of epileptic and encephalitic cases, the treatment of cardiopaths, Braille work for the blind, sight-saving instruction for the partially sighted, and the expansion of many topics whose treatment is all too brief.

It is not clear why the title of the book is limited to the "psychology" of exceptional children since it contains much that is not psychological. To the reviewer its place seems to be limited to the general student in psychology or education who merely wants a brief overview of the field without any thought of specializing in its clinical, social, or educational phases. As a text its value will be enhanced if the course is offered by a specialist who can supplement the book with extensive first hand experience. Most of the questions at the chapter ends are excellent. The author has shown considerable versatility and skill in the construction of varied and provocative exercises.

Delaware Division of Mental Hygiene J. E. W. WALLIN

Biographies of Child Development: The Mental Growth Careers of Eighty-Four Infants and Children. By ARNOLD GESELL, BURTON M. CASTNER, HELEN THOMPSON, and CATHERINE S. AMATRUDA. New York, Paul B. Hoeber (Medical Book Department, Harper & Bros.), 1939. Pp. xvii, 328.

This latest book from the Yale Clinic of Child Development consists almost entirely of case histories. Part I written by Dr. Gesell, deals with a group of 30 cases which were first reported 10 years ago (A. Gesell, *Infancy and Human Growth*, 1928). These same children have been examined further to determine to what extent the predictions formulated when they were infants are consistent with the diagnoses 10 years later. The bulk of the report consists of the detailed findings for each of the children. In the majority of these selected cases it was found that the predictions were fairly accurate.

The second part of the book, written by the various members of the staff, is concerned with special problems. Here also the predictions made during infancy are compared with later diagnoses. These special problems include the superior child (Thompson), language and reading problems (Castner), irregular mental development (Castner), twins (Gesell), foster care and adoption (Amatruda), and children with physical complications (Amatruda).

The book includes descriptions of children in which the early predictions were later verified, showing what can be done in the way of predicting, and it also includes descriptions of children whose later development was inconsistent with the early predictions. In these latter cases the authors attempt to analyze the reasons for the incorrect prediction or change in the rate of development. In general in the cases cited the superiority or inferiority of a given child was usually predicted accurately if the home conditions were good, but prophesies were unreliable in those cases in which the children were brought up under adverse conditions. Accurate predictions were often made far in advance. For example, superiority or difficulty in language or reading was often prophesied before the child was old enough to talk or read.

There is no indication anywhere in the book of the total number of cases which were predicted correctly or incorrectly, and the cases described were selected, as Gesell mentions in his introduction, not because they were typical but because they taught the members of the staff the most. Therefore these cases do not give a typical picture of development, and the study is of no value except as a comparison of the individual cases described.

The book is of interest to those who have followed the earlier work at Yale and should be considered merely as a supplement to the earlier volumes, not as an independent work or general treatise on child development.

Cornell University MARY S. RYAN

Pintner General Ability Tests: Verbal Series. By RUDOLF PINTNER, BESS V. CUNNINGHAM, and WALTER N. DUROST. New York, World Book Co., 1939.

This series consists of primary, elementary, intermediate, and advanced group tests, which are all available in two equivalent forms. The Pintner-Cunningham Primary Test is for kindergarten, grade 1, and the first half of grade 2, and is published in forms of seven subtests each, composed entirely of pictures which are marked by the subjects according to the examiner's verbal directions. Form A of the Primary Test is like the original Pintner-Cunningham Primary Mental Test except that the pictures have been redrawn and new items have been substituted for dated ones. The Pintner-Durost Elementary Tests for which norms are now being developed is designed for the last half of grade 2, grades 3 and 4. The Intermediate Test is for grades 5 to 8 inclusive and each form consists of 8 subtests: vocabulary, logical selection, number sequence, best answer, classification, opposites, analogies, and arithmetic reasoning. All answers are of the five alternative, multiple-choice type. The Advanced Test is similar to and an extension of the Intermediate. Working time for the various tests ranges from 30 to 55 min.

A well prepared manual of 16 pages for the Intermediate and Advanced Tests gives a description of the series, a brief but unusually adequate statement of the requirements of a good test, characteristics of statistical data and standardization of the series, and suggestions for interpretation and use of the tests. A nomograph

for facilitating conversion of standard scores to *IQs*, a procedure which is justified by the authors on the basis of refinements in statistical analysis and completeness of their norms, is appended to the manual. Directions for administering and scoring which have been carefully edited are given in a separate manual.

Sampling procedures as well as number of cases used in establishing the norms (the total is above 100,000 *Ss*) would seem to be satisfactory. Every child in the public schools in the appropriate grades of the coöperating communities took the test, and the authors believe they have eliminated sampling errors which might arise from intracommunity selection. Communities were chosen which were widely different in size, socio-economic status, and geographic location.

The eight subtests of the Intermediate and Advanced batteries measure several psychological characteristics which are statistically somewhat independent. When the verbal factor is kept constant the median partial intercorrelation of the subtests is 0.30 for the Intermediate and 0.26 for the Advanced.

Statistical reliability of total scores on the Intermediate Test, Form A correlated with Form B, is 0.94 based on 101 cases (odd-even reliability is 0.94 with $N = 101$). The interform reliability of the Advanced Test, based on 92 cases, is 0.84 (odd-even reliability 0.90 with $N = 587$). Reliabilities for the total tests are high enough for group comparison but test performance must of course be interpreted with great caution when the purpose is individual diagnosis or orientation. Probable errors of measurement, expressed in terms of standard scores, are 2.6 points for the Intermediate and 3.9 points for the Advanced.

Reported statistical validity of the tests is very high. The Intermediate Test correlates 0.84 with total scores on the Metropolitan Achievement Tests for 168 cases in grade 5, and 0.79 for 209 cases in grade 7. Coefficients between the Pintner General Ability Tests and seven other psychological tests range from 0.71 for the Revised Stanford Binet to 0.87 for the Otis Group Intelligence Scale.

The reviewer was favorably impressed, not only by the care used in the development of the tests, their elaborate standardization, and their detailed evaluation *before* publication, but also by the clear and conservative presentation of interpretative material in the manual. Repetition of such statements as the following quotation from the manual will do much to aid test users in general to appreciate limitations of adequate instruments of measurement—"success or failure must be interpreted in terms of a variety of additional facts concerning the child."

University of Maryland ROGER M. BELLOWS

The Effects of Varied Amounts of Phonetic Training on Primary Reading. By DONALD C. AGNEW. Durham, Duke University Press, 1939. Pp. viii, 50.

In an introductory chapter Dr. Agnew points out the *a priori* nature of statements concerning the value of phonetic training, discusses the disagreement between teachers and writers of textbooks, and stresses the need for additional experimental investigations of the problem. The following six chapters are devoted to an exposition of two investigations made in the primary schools of Raleigh and Durham.

The Raleigh study was made to determine the relative value of phonetic and non-phonetic training as a basis for teaching reading abilities. Groups of third grade pupils were equated for *MA, IQ,* and rate of progress in school, but differed in amounts of phonetic training as determined by teachers' rating scales. Group tests of silent reading abilities and vocabulary, and individual tests of oral reading, word

pronunciation, eye-voice span, and phonetic abilities were administered. The differences in test performance between the groups were small and not consistent.

It was thought that the small differences in amounts of phonetic training between the groups, or the interference of one type of training with another due to the inconsistency of phonetic training in the school grades, might have had some influence on the results. Phonetic training in the Durham schools had been greater in amount and more consistent than in Raleigh, so the second study was undertaken there as a check on the first. When groups of Durham and Raleigh pupils were compared, performance of the former was significantly higher on all tests except those of speed and accuracy in silent reading, indicating that intensive phonetic training does improve certain reading abilities. In a final chapter the results are summarized and certain implications are discussed. A bibliography of 47 titles is appended.

One is impressed by the caution with which Dr. Agnew generalizes and by his control of many variables. In a field which is replete with generalizations of unknown dependability and with *a priori* conclusions he has not attempted a final answer to the questions involved, but has stressed the necessity of obtaining adequate criteria for the teaching of primary reading before advising extensive use of phonetic training.

University of Maryland JOHN W. MACMILLAN

We Call It Human Nature. By PAUL GRABBE. In coöperation with Gardner Murphy. New York, Harper & Bros., 1939. Pp. 120.

This attractive little book represents an attempt at an accurate but popular account of the highlights of scientific psychology by an author who is not himself a psychologist. He has here utilized methods of presentation which had been developed previously for the teaching of languages. His factual material was obtained under the guidance of Gardner Murphy, who has apparently given freely of his time and assistance.

Grabbe's approach to his subject consists of a very terse text, liberally illustrated with diagrams, cartoons, and photographs. Most impressive are the photographs, which are excellent, both pictorially and in subject matter. The diagrams are designed to provide schemas of various psychological concepts and functions. These drawings make use of certain special symbols which represent "organism," "stimulus," "drive," "response," and the like. For full understanding, rather careful reading with adequate reference to the glossary is necessary.

If shortcomings must be pointed out, we may begin with the condensation of the text. The process of boiling down has progressed to the point of virtual stylelessness, which makes for rather choppy reading. Another result of this overcondensation is a relative lack of clarity. One wonders what the naïve reader, to whom this volume is addressed, will do when confronted with the technical terms employed by Grabbe. Perhaps twice as many words would have resulted in a book more readable. Further, some will note the lack of any sensory material whatsoever. Could not colors, sounds, and even flavors be both interesting and important to the reader? Should *all* learning be treated as though it were conditioning? Why, after a purely naturalistic development of the subject of psychology, does the author so uncritically present psychoanalytical constructs as proven truths?

Even if the above objections are valid, one can, with few misgivings, recommend

this work to someone who wants to read a book, not too long, which will tell him something of what psychology is about.

University of Alabama — F. NOWELL JONES

Human Nature Writ Large. By F. CREEDY. Chapel Hill, University of North Carolina Press, 1940. Pp. 484.

This is an attempt to look at Occidental civilization with a detached objectivity, comparable to the anthropologist's examination of a native culture far away or long ago. A central thesis is that there are in human nature certain primal urges—self-preservation, reproduction, coöperation, creative intellectual activity—necessary to survival, whose realization in a non-conflicting way must, therefore, be the purpose of a human society. The expressions of these urges get conventionalized and fixed in the mythologies of theistic religion, of nationalism, of a fictitious free economic competition, of sexual taboos, of romantic love; and these become effective in establishing "crowd habits" through the exaltation of certain personal heros to dramatize the myths, the crystallization of the myths in art and literature, and the loosing of an abundance of catchwords or "trigger phrases" to touch off the unthinking popular prejudices. The general reader will find the book refreshing in its very independence of those conventionalities and its critical and constructive suggestions.

The psychologist will not be impressed with the references to his working concepts and tools. The 'basic human urges' are apparently traditional ones, and show no hint of original observations nor of contact with more recent laboratory studies. The treatment in antithesis of 'habit' and 'attitude' is not in line with the recognized variability of habit, nor the truly habit-character of attitude. Definitions offered or projection,' 'transference,' and the like, leave much to be desired; nor is it true to speak of them as having been defined by 'introspections.' The frequent references to 'psychic conflict' are not enlightening. The method of 'behavioristic psychoanalysis by means of documents' is such only in a figurative way: it is essentially a detailed sentence-by-sentence syllogistic analysis of a text to make explicit its implicit assumptions. All in all, the psychological foundations of the work are uncertain and shaky. It seems that the author has not writ human nature in the large, but has attempted to come the other way—from his observations of social phenomena to check their foundations in the human nature of the individual.

Apparently much more command is shown of the economic field; and the book remains stimulating in its many suggestions there and in other matters of public concern.

University of North Carolina — JOHN F. DASHIELL

Tests and Measurements in the Improvement of Learning. By ERNEST W. TIEGS. Cambridge, Mass., Houghton Mifflin Co., 1939. Pp. xxi, 490.

Designed as a text for classes in tests and measurement, this book is an extension of the author's earlier *Tests and Measurements for Teachers* (1931). The emphasis on "approaches based more closely on the activities and problems of the educational program" attempts "to meet the measurement problems and needs of teachers on different levels as well as to provide data for the solution of supervisory and administrative problems." The first four chapters give a straightforward and interesting

discussion of measurement, use, and meaning of the IQ; and construction of the various types of "informal" diagnostic devices. 'Informal' seems in this usage to be synonymous with 'standardized' and one might take serious issue with the author for urging such judgment-making devices on the teacher if he had not stressed the frequent use of such casual objective tests primarily as means of diagnosing student difficulties interfering with mastery of clearly-stated objectives. The danger of having teachers come to rely on such *diagnostic* devices as *appraisal* devices must be pointed out.

The next six chapters deal with diagnosis and appraisal in primary learning, elementary grades and secondary schools and colleges. This treatment, according to levels of education, leads to repetitious and cursory summaries of areas of diagnosis, even though it provides a convenient reference for teachers interested in a specific level. For example, diagnosis of reading is discussed in three different places, rather than in one coherent and unified chapter. The sections on personality and interest diagnosis are the least critically discussed throughout these chapters, with inadequate mention of the specialists who can and do contribute aid to the teachers in these areas. Furthermore, the tests used for illustration are not discussed critically in terms of their own reliability or validity.

Chapters on ability-grouping, curriculum-building, teaching effectiveness, marking and promotion, and planning and interpreting the measurement program pick up the interesting and informal style of the opening chapters. Essential statistics for the teacher are included in two chapters of extremely rapid development by listing and illustrating formulae and their use.

As minor criticisms, the index is not well organized; the selected bibliographies carry occasional errors in authors' names; throughout the nineteen chapters ideas and concepts are too frequently repeated. On the whole, however, teachers in training and in service should find much of the book stimulating and helpful.

University of Minnesota JOHN G. DARLEY

The Health of College Students. By HAROLD S. DIEHL and CHARLES E. SHEPARD. Washington, D.C., American Council on Education, 1939. Pp. 169.

Since 1907 the student health services in our colleges and universities have been frequently surveyed and the results published. Nine such surveys have revealed such a wide variation in these services that the development of a practical set of standards has not been forthcoming. Two National Conferences on College Hygiene (one in 1931, the other in 1937) attempted to set up minimal standards without any special consideration of the size and type of the institution. This report by Diehl and Shepard of their tudy of 549 colleges and universities attempts not only to summarize the main facts regarding student health service and college health teaching, but it also makes definite suggestions as to the organization and development of these activities: (1) in all colleges; (2) in public institutions—except teachers' colleges—large private coeducational institutions, women's colleges, and large colleges for men; (3) in teachers' colleges and colleges with enrollment under 1,000; and (4) in colleges with day students almost exclusively.

Though based upon data derived from a questionary and subject, therefore, to the definite limitations of that type of research, this report gives us in its 169 pages a surprisingly clear picture of the efforts our colleges and universities are making

to protect and improve student health. We may question the recommendation that a Wassermann test be required as a routine part of every college health examination. We may wonder why these authors recommend one college health physician to every 800 students, while the National Conference on College Hygiene recommended one to every 500 students. We may wonder if the accuracy of the data justified the rather involved statistical treatment given them. The authors have, however, succeeded in presenting very plainly what present student health service practice is at the present, and what it ought to be in the future.

Cornell University D. F. SMILEY

A Study of Jealousy as Differentiated from Envy. By T. M. ANKLES. Boston, Bruce Humphries, 1939. Pp. 109.

The present volume was presented as a part of the examination for an Academic Postgraduate Diploma in Psychology at the University of London. It would appear that the A.P.D.P., which the author writes after his name, is roughly equivalent to the M.A. or M.S. degree in America. The publication under consideration is approximately of the calibre of the average Master's thesis, yet the book has previously been published in England, and the jacket cover advertises this printing as the *first* American edition.

Mr. Ankles' research consisted in interviews of 30 subjects, selected at random from "not-too-intimate acquaintances" and from among persons met across the tea-table in public restaurants. Mr. Ankles presents separately the result of each interview.

Mr. Ankles has humanitarian as well as scientific interests, as is shown by the final paragraph of Case Study No. I:

"Before dismissing Mr. A., it is well to say that part of the bargain for his interview was that I make plain to him the reason for his homosexual attachment. Mr. A., it was found, loved his mother intensely as a youth, but was unrequited, so fixated on his father, and all women to him (unconsciously) are 'his mother' multiplied. This cause being made clear, Mr. A. promised to try to see things in a different light."

There is no table of contents, but this is scarcely surprising, as the book is not divided into chapters. There is no bibliography, but in the text there are occasional references to Adler, Freud, Briffault, Darwin, and Watson.

University of Virginia WAYNE DENNIS

This Business of Living. By L. W. GRENSTED. New York, Macmillan Co., 1939. Pp. 185.

This book discusses science, art, and religion from a common sense standpoint in order to indicate what each may contribute to the solution of everyday problems of living. Dr. Grensted is Nolloth Professor of the Christian Religion at the University of Oxford. Aside from the fact that it is refreshing to find a British divine keeping more or less up to date in psychology, the book has but little interest for the professional psychologist. His discussion of science is commonplace and limited in its point of view, that of art is based upon a single theory, the intuition of values, which, like all the other esthetic theories, is of much disputed validity. His discussion of religion is by far the best part of the book, and seems to a layman to be of considerable practical value. He characterizes religion as that which "constitutes for

each one of us the guiding principle by which the business of living is directed." This may be pleasure or the satisfaction of appetites, money making, power, the welfare of men, truth, beauty, goodness, holiness, etc.; and for each of these there is a god, which, in the business of living, is the actual object of worship of various groups of men.

Hobart College FOSTER P. BOSWELL

Textbook of Abnormal Psychology. By R. M. DORCUS and G. W. SHAFFER. 2nd ed. Baltimore, Williams & Wilkins Co., 1939. Pp. xvii, 475.

The first edition of this volume won many commendations, including the present reviewer's, and Knight Dunlap is probably right in crediting the present revision with maintaining its first place among texts on abnormal psychology. It is comprehensive, and bears witness to painstaking search of literature, as well as personal contacts with the field. There are two notable drawbacks, both looking like the result of hurried preparation. The presentation is not sufficiently articulated to spare an instructor a considerable burden of further organizing it for the learner. Again, some might refuse to call it notable but it is real, the literary quality is scarcely at par; there are passages that seem lifted bodily from informal classroom discourse; *e.g.* use of the pronoun *I* (p. 103) in a work of joint authorship. Examples of terminological looseness are the definitions of neologisms (p. 93) and of aphasia (p. 107). A mental age reaching up to 12 yr. (p. 351) is not, and should never have been, an acceptable characterization of "moron;" and what can be the reason for printing, in a book dated 1939, a three page summary of the 1916 Stanford-Binet (pp. 354-357), while according to the 1937 revision three lines?

In the future editions through which this book will doubtless and deservedly pass, time and trouble should be taken to guard against remediable weaknesses such as these. Perhaps the work would be more useful if organized as a source-book, for which the authors appear amply equipped. Without in any way prejudicing the present book's position as meritorious and the best in its field, it has still some ground to cover before it invites, from detached judgment, all the laudatory foreword by Dunlap, or comes within hailing distance of Chapman's opening appraisal.

Harvard University F. L. WELLS

Le mystère animal. By DANIEL ROPS AND OTHERS. Paris, Librarie Plon, 1939. Pp. ix, 301.

This little volume is a curious mixture of prose poems, essays, and scientific surveys by students of animal life. The book is divided into two parts. The first part, entitled "Animals and Men Today," has little scientific value. It contains: "Animals I Have Known" by Colette; "The Love of the Animals" by Edmond Jaloux; "The Wild Animal and Man" by André Demaison; "Portraits of Domestic Animals" by Charles Silvestre; "What Man Owes to the Animal," by Jean Canu; and "The Inhuman World and the Love of the Animals" by Daniel Rops.

The second part, entitled "From Animal to Man," begins with an article on "The Brain of the Animal and That of Man" by Walther Riese—a valuable survey of the present theories of neuro-physiology. This is followed by Eduard Claparède's article "From Animal to Human Intelligence"—an excellent survey of the recent results in animal psychology. Less good is the essay by Paule Règnier on "Animal and Human

Pain." The most interesting chapter of the whole volume is that by Marguerite Combes and Charles Boucaud on "Animal and Human Society." At the end is a worthy sermon, "From Animal to Man: The Mystery of the Soul" by Abbè Jean Plaquevent which is written from the point of view of the Roman Catholic Church.

New York City ERNST HARMS

Conscious Orientation. By J. H. VAN DER HOOP. New York, Harcourt Brace & Co., 1939. Pp. vii, 352.

Conscious Orientation is a detailed exposition of the various types of human mental structure from a phenomenological point of view. Unfortunately the author's psychiatric experience has not served to anchor the theories to the dynamic social relations which they attempt to explain. In his systematic presentation, Dr. Van der Hoop draws most heavily on the personality typology of Jung, though he differs from him in such respects as insistence on a distinction between feeling and emotion, and between sensation and perception. It thus becomes possible for the author to extend the types of possible variations of extraversion and introversion.

This attempt at "a study of personality types in relation to neurosis and psychosis" bogs down because of the too tenuous lines which are drawn between psychology, metaphysics, and philosophy. The writing (or it may be the translation is inclined to be excessively wordy, very rambling and overconcerned with minutiae. There is throughout the tendency to regard abstractions of mental phenomena as if they were functioning entities.

The author takes a step in the right direction when he refuses to view the contributions of Jung and Freud as necessarily incompatible because they represent different schools. The Jung-Freud synthesis which he presents is not, however, especially fortunate.

Conscious Orientation will not offer much to the systematic student in his search for a workable typology. This is so primarily because of the work's essential non-dynamic view concerning the mechanics of personality development, and its dependence upon vague concepts of "inherited predispositions," "instinctual life," "intuition," and "feeling types." Despite its occasional stimulating discussion of the current systems of classification, the book seems more like a curious historical relic of naïve mentalism, than a psychological treatise offered in 1939.

Brooklyn College ROSALIND GOULD

Gausteine zur Psychonalyse. By SANDOR FERENCZI. Postume works edited by Vilma Kovacs. Bern, Hans Huber, 1939. Vol. 3, pp. 543; Vol. 4, pp. 411.

Sandor Ferenczi, highly rated as Freud's most eminent disciple, died in 1933. In 1926 he had issued two volumes of studies under the title *Bausteine zur Psychoanalyse,* to which friends have now added two further volumes which contain the scientific heritage of this doubtless very able psychiatrist.

Vol. 3 contains original studies from the years 1908-1933, among which the papers "Psychoanalyse und Paedagogie" und "Entwicklungsziele der Psychoanalyse" are known as the best of his contributions to his movement. Vol. 4 reprints criticisms and referats, fragments and important notes, as well as a bibliography listing over 300 publications.

New York City ERNST HARMS

Mastering your Nerves. By PETER FLETCHER. New York, E. P. Dutton, 1939. Pp. 241.

This book is mainly a discussion of functional as opposed to organic processes in borderline mental difficulties, and is especially intended to give patients insight into their own problems. The first part of the book is analytical and the second part is a practical guide to help the reader "build up a new strategy of living." This book does not pretend to offer new approaches or theories, nor does it review advances in psychomedical science. It is a clear and practical discussion of principles familiar to psychologists, and intended for readers for whom such principles are neither clear nor familiar. The author's basic ideas are quite commonplace but sound, avoiding cumbersome and dubious assumptions, especially those of psychoanalysis. The author enlivens his pages with case histories and occasionally with shrewd insights.

Wells College — C. O. WEBER

About People. By WILLIAM STEIG. New York, Random House, 1939. Pp. 105.

"Steig has something new and important to say about the unconscious as it is related to the world we know." Even if psychologists are somewhat hesitant about accepting at fact value the publisher's statement on the dust-jacket, they cannot fail to be amused and stimulated by the 105 drawings in this thin volume. There is no verbal commentary beyond the title and, indeed, none is needed. 'The Affable Man' is portrayed as a top with a rounded bottom, bobbing back to vertical no matter how struck. 'Persistent Thought' depicts a man swinging pendulumwise on a rope that binds him hand and foot. 'Anxiety' is represented by a mannikin attached to the head of an unfortunate who peers about with a worried air. It would not be an exaggeration to describe the book as presenting a series of epigrams in the form of line drawings.

University of Maryland — JOHN G. JENKINS

The Nature of Creative Activity. By VIKTOR LÖWENFELD. New York, Harcourt, Brace & Co., 1939. Pp. xvii, 272.

The author conducted an elaborate series of experiments with a group of children having decided visual defects, either physical or mental or both, and compared them with a corresponding group of children with normal vision. His procedure was to ask the child to represent a specific situation graphically, and he publishes here a very extensive series of drawings from different ages and different types of artistic activity.

His main contention is that the artistic experience does not necessarily have to be an experience of the sense of sight but can be haptic in origin. He accordingly describes two types: the visual and the haptic, the latter being strikingly illustrated in the drawings of those who have defective vision but which may also be shown by those who have normal vision.

He divides art forms into two large groups; the impressionistic and the expressionistic. "The impressionistic world is the world of appearances, the world of our senses. The world of expressionistic art is the world of expression, of feelings of subjective processes."

The book is a valuable contribution to the general theory of esthetics.

State University of Iowa — CARL E. SEASHORE

The Psychology of Conversation. By T. H. PEAR. London, New York, Thomas Nelson, 1939. Pp. lx, 171.

This little volume appears as a number 51 of a popular series (Discussion Books). Despite this fact it not only is written upon a high scholastic level, but its argument is excellently supported by experimental results gleaned from various technical psychological journals. It is also furnished with a bibliography of 58 items.

A list of the chapter headings indicates the scope of the work: I. Questions about Conversation; II. The Art of Conversation; III. Conversational Tact; IV. Conversations with Oneself; V. Remembering Conversation; VI. Subjects of Conversation; VII. Children's Conversations; VIII. Techniques of Interviewing; IX. Conversation in Psychotherapy.

It is a sufficient evaluative comment upon this book to remark that in general plan, style, and content it admirably serves the purpose for which it was intended.

Indiana University — J. R. KANTOR

Rural Youth: Their Situation and Prospects. By BRUCE L. MELVIN and ELNA N. SMITH. Washington, D.C., U. S. Gov. Printing Office, Res. Monog. 15, Div. Soc. Res. W. P. A., 1938. Pp. xx, 167.

This monograph presents a most comprehensive survey of the present situation and future prospects of rural youth. Sociologists and psychologists alike will find the account of the inadequate economic opportunities, of the equally inadequate educational and recreational facilities a necessary base for the understanding of the problems of present-day youth. Any discussion of guidance and counseling techniques will naturally best stem from a thorough knowledge of the actual areas of living in which difficulties are most likely to be experienced. Some attempts to meet youth's problems are discussed. These attempts reveal the necessity for guidance and education if we are to have any maintenance, much less progress, of the citizenry standards of today.

Brooklyn College — ROSALIND GOULD

Rural Families on Relief. By CARLE C. ZIMMERMAN and NATHAN L. WHETTEN. Washington, D.C., U. S. Gov. Printing Office, Res. Monog. 17, Div. Soc. Res. W. P. A., 1938. Pp. xxiv, 161.

Psychologists will find less direct interest in this monograph. The statistical survey is adequate and clearly presented, but only hypothetically can one deduce the psychological effects of migratory existence, of low status, of a narrowly restricted concept of the 'future,' upon the family heads and the development of their children, from these data. A clinical survey of the effects upon family relationships and the 'growing child' of protracted existence on relief would be most valuable. Sociologists would probably be interested in the important implications of these data for future social relations.

Brooklyn College — ROSALIND GOULD

Avoir une âme: Essai sur les existences virtuelles. By ETIENNE SOURIAU. Paris, Annales University of Lyon, 1938. Pp. 141.

This book is a combination of philosophy and *belles lettres.* It contains little of interest to scientific psychology.

The Rice Institute — F. A. PATTIE